THE

ECLECTIC REPERTORY

AND

ANALYTICAL REVIEW.

VOL. X. JULY, 1820. No. III.

SELECTED PAPERS.

A Statement of Facts tending to establish an estimate of the true value and present state of Vaccination. By SIR GILBERT BLANE, Bart. M. D. F. R. S. Physician in Ordinary to the Prince Regent.

[From the Medico-Chirurgical Transactions, Volume X.]

IT is now twenty-one years since Vaccination was promulgated in this country by Dr. Jenner, and fifteen years since it began to produce a sensible effect in diminishing the mortality from small pox. In regard to the latter period, it is coeval with this society; yet, though no discovery in nature nor in medicine has been more important to the interests of humanity, nor any which ever so rapidly and universally has won the assent and practical adoption of mankind, there are no notices of it on our records, except some allusions to its influence on other disorders, in our second and third volumes. As it is to be hoped that our labours will prove to posterity some of the principal sources of reference regarding the medical and chirurgical discoveries and improvements of the age; as it is one of the reproaches of the country that it has not availed itself so much as any other of the benefits of vaccination; and as there are writers among us who still allege that the failures are so numerous that the value of the discovery is very ambiguous,

it seems one of the duties of the society to lend its aid in placing these important points in their true light.

It seems almost needless to premise, that the small pox is of all maladies that, which, during the last thousand years, has destroyed the largest portion of the human species, and been productive of the largest share of human misery. There is, perhaps, no disease over which medical art has less power; and this power, such as it is, has consisted more in abolishing pernicious practices than in ascertaining any positive methods of controlling its fatality; unless we except the inoculation of it with its own *virus*. But, though the beneficial effect of this on those on whom it is actually practised is undeniable, it has no tendency like vaccination to extirpate the disease; and from the impossibility of rendering it universal, it has actually been found to add to the general mortality of small pox, by opening a new source of diffusion to its *virus*.

In order to bring this to the test of calculation, in order also to institute a comparison of the mortality of small pox as influenced by vaccination, as well as by inoculation from itself, I have selected from the bills of mortality four periods, each of fifteen years, for the purpose of exhibiting the mortality of small pox in each of these series in regard to each other. These are thrown into the form of tables, and annexed to this article.

The first series is the fifteen years immediately preceding the introduction of inoculation, that is, from 1706 to 1720, both included. Previous to this period, no account that could be depended upon regarding the small pox, could be derived from the bills of mortality; for down to the beginning of last century such was their imperfect construction, that small pox, measles, and flux, were blended under one head. Exception may be taken against the accuracy of these bills, even in this improved state, particularly with regard to the discrimination of diseases. This objection, however, is certainly less applicable to small pox than any other disorder, its character being so striking as not to be mistaken by the most ignorant and careless observer.

The second series is taken at the middle of the last century, when inoculation had made considerable progress, that is,

from 1745 to 1759, both included. In comparing this with the preceding series, with regard to absolute numbers, it ought to be taken into account, that eleven parishes were added to the bills of mortality, between the years 1726 and 1745, both included: so that the progressive improvement of general salubrity ought to be estimated still higher than what is indicated by the diminished mortality, as it stands in the tables.

The third series comprises the fifteen years previous to the introduction of vaccination, when inoculation had made still greater progress; that is, from 1785 to 1798, both included.

The fourth series comprises the time in which the vaccine inoculation has been so far diffused as to produce a notable effect on the mortality of small pox; that is, from 1804 to 1818, both included.

The result of these computations stands as follows:

Ratio of the Mortality of Small Pox to the total Mortality.

From 1706 to 1720, one in 12.7; that is, 78 in 1000.
From 1745 to 1759, one in 11.2; that is, 89 in 1000.
From 1785 to 1798, one in 10.6; that is, 94 in 1000.
From 1805 to 1818, one in 18.9; that is, 53 in 1000.

In all these computations, fractions are not noticed in the last column of numbers.

It appears from this statement, that the proportion of deaths from small pox to the total mortality, increased in the course of last century; so that inoculation appears to have added to the mortality. It is but fair to mention, however, that this total mortality is not quite a just scale whereby to measure the relative mortality of small pox; for in the course of that century, the general mortality itself was greatly diminished in relation to the population. This diminution of general mortality was chiefly owing to the diminished mortality of children under two years of age, which, at the time when the account began to be kept, 1729, averaged about 9000; but at the end of the century not more than 5000; also to the decrease of fevers, and still more of fluxes. The relation of the mortality of small pox to the population, would therefore be a more fair criterion of its increase or decrease. In this

according to the best computation that the *data* afford. It will be seen by an inspection of the table, that, in that time there have been great fluctuations in the number of deaths. This has been owing partly to the small pox inoculation of out patients having, by an unaccountable infatuation, been kept up at the small pox hospital for several years after the virtue of vaccination had been fully confirmed. The great number of deaths in 1805 may chiefly be referred to this cause. Since the suppression of this practice, the adoption of vaccination, though in a degree so incomplete, in consequence of public prejudice, created by mischievous publications, has been unable to prevent a considerable, though fluctuating, mortality from small pox. This mortality, though little more than one half of what it was in former times, might have been entirely saved, if vaccination had been carried to the same extent as in many cities and whole districts on the continent of Europe, in Peru* and Ceylon.

It is now matter of irrefragable historical evidence, that vaccination possesses powers adequate to the great end proposed by its meritorious discoverer, in his first promulgation of it in 1798, namely, the total extirpation of small pox. The first proof of this was at Vienna, where, in 1804, no cases occurred, except two strangers who came into the city with the disease upon them. In 1805 there did not occur a single death from it in Copenhagen.† Dr. Sacco, the indefatigable superintendent of vaccination in Lombardy, stated, in his Annual Report, 3d January, 1808, that the small pox had entirely disappeared in all the large towns in that country; and that in the great city of Milan it had not appeared for several years.

* In the summer of 1811, the author was called to visit, professionally, Don Francisco de Salazar, who had arrived a few days before in London, on his route from Lima to Cadiz, as a deputy to the Spanish Cortes. He informed me, that vaccination had been practised with so much energy and success in Lima, that, for the last twelve months, there had occurred, not only no death from, but no case of, small pox; that the new-born children of all ranks are carried as regularly to the vaccinating house, as to the font of baptism; that the small pox is entirely extinguished all over Peru; nearly so in Chili; and that there has been no compulsory interference on the part of the government to promote vaccination.

† See Pfaft neun nord v. Archiv. B. I.

Dr. Odier, of Geneva, so favourably known for his high professional, scientific, and literary acquirements, testifies, that after a vigorous perseverance in vaccination for six years, the small pox had disappeared in that city and the whole surrounding district; and that, when casually introduced by strangers, it did not spread, the inhabitants not being *susceptible*. The Central Committee in Paris testify, in their report of 1809, that the small pox had been extinguished at Lyons and in other districts of France.

These are selected as some of the earliest proofs of the extirpating power. But it is demonstrable, that if, at the first moment of this singular discovery, at any moment since, at the present or any future moment, mankind were sufficiently wise and decided to vaccinate the whole of the human species who have not gone through the small pox, this most loathsome and afflicting of all the scourges of humanity would instantaneously, and for ever, be banished from the earth.

It is farther manifest, that extirpation being the sole and ultimate aim of this discovery, and there being the fullest historical and practical evidence of its being capable of accomplishing this end, all other questions with regard to its expediency must be futile and irrelevant. It is in the nature of all morbid phenomena, to be liable to exception. One of the most essential and characteristic laws of small pox itself, namely, that of its affecting the human subject but once in life, is found, in rare cases, to be violated. It is, therefore, perfectly conformable to analogy, and naturally to be expected, that it may not in all cases be a complete security against small pox. But it is obvious, that, admitting these exceptions to be very frequent, much more so than the recurrence of small pox after small pox, this can constitute no objection to the practice, as long as the extirpating power remains unimpaired and unimpeached. Nay, it is obviously so far from an objection, that it ought to operate as a powerful additional incentive on every benevolent mind, to push vaccination to the utmost, as rapidly as possible; in order that those who are still susceptible, either from peculiar natural constitution, or from the unskilful manner of conducting the operation, or from defective matter, may not, by any possibility, catch it; for, in the event

of its extirpation, it could no where be met with. And in order to stimulate the good and the wise to aim strenuously at this consummation, let it be constantly borne in mind, that the adversary they are contending with, is the greatest scourge that has ever afflicted humanity. That it is so, all history, civil and medical, proclaims: for, though the term plague carries a sound of greater horror and dismay, we should probably be within the truth, if we were to assert, that small pox has destroyed a hundred for every one that has perished by the plague.

It is true, that in its last visitation of this metropolis, one hundred and fifty-four years ago, it carried off 70,000 victims in a few months; but since that time, the deaths from small pox, recorded in the bills of mortality, have amounted to more than 300,000; and a like number of the survivors have been afflicted with blindness,* deformity, scrofula, or broken constitutions, which is not the case with the plague. And when it is considered that there are large portions of the globe, India, China, even one whole quarter of it, North and South America, besides all the tropical and arctic regions, in which it has never been known; and that in all the countries liable to it, it seldom appears but at one season of the year, and sometimes at long intervals, the ravage which it makes is trifling when compared with the unceasing havoc of small pox, which spares no nation in any climate, or at any season.

The preceding reasoning is grounded on the supposition of extirpation; but, however demonstrable the *possibility* of extirpation may be, it may not in all communities be *practicable;* and may not these alleged failures so operate, as, in such cir-

* It appears by a report of the hospital for the indigent blind, that two-thirds of those who apply for relief have lost their sight by the small pox. It is alleged by some of the soundest political economists, that small pox does not diminish the numbers of mankind, nor vaccination increase them; for population is determined by subsistence, and the indefinite powers of procreation soon repair the ravages of disease. But, however this may be, the miseries incident to so many of those who survive small pox, whereby they become a burden to themselves and to society, render this disease uncontrovertibly an evil of the first magnitude, not to speak of the intense sufferings and afflictions inseparable from it.

cumstances, to render the expediency of the practice questionable?

In order to decide this, let the nature and amount of these failures be ascertained and estimated.

The description of those cases of small pox, if they can be called so, which occur in vaccinated subjects, is shortly as follows. The invasion and eruption in every respect resembles that of the genuine small pox. I have seen it attended with high fever and a thick crowded crop of *papulæ*, such as precedes the most severe and dangerous cases of the confluent kind. This runs on till the fifth day from the eruption, both days included, at which time some of the *papulæ* begin to be converted into small sized pustules. The disorder then abruptly stops short. On the following day the fever is found to have subsided, with a shrivelling and desiccation of the eruption, and recovery proceeds without the least danger or inconvenience. The face is marked, for some time after, with brown spots, but without pits. It should never be forgotten, that all morbid *phenomena* are full of varieties and exceptions. Accordingly, though the fifth day is the most common limit of this disorder, it sometimes stops short on the third; sometimes not till the sixth or seventh; and in a very few cases it has been known to run the common course of small pox. What forms the strong line of distinction from proper small pox, is that, with a few exceptions, it does not proceed to maturation and secondary fever, which is the only period of danger. I am not prepared to deny that death may not have occurred in a few instances, nay, there seems sufficient evidence that it actually has; but these adverse cases are so rare, as not to form the shadow of an objection to the expediency of the general practice. A few weeks ago, at a meeting of this Society, at which forty members and visitors were present, I put the question whether any of these eminent and extensive practitioners had met with any fatal cases of this kind. Two gentlemen had each seen a single case, and two other gentlemen took occasion to say that they had each seen a case of second small pox, both of which proved fatal. It is evident, therefore, that according to that maxim which guides mankind in the conduct of life, namely, that of acting on a general rule and ave-

rage, and not on exceptions, these adverse instances ought not to have the least influence on practice, even though they were much more numerous.

As it is of the utmost consequence to establish the strong and important distinction between small pox, properly so called, and that which takes place after vaccination, which may be called the mitigated, or five-day small pox, a few of the most impressive testimonies respecting the safe nature of the latter may be here recited. Mr. Brown,* of Musselburgh, gives the detail of forty-eight cases, in none of which did the secondary fever or death occur. Here was a saving of at least eight lives, at the lowest computation, for this is the number which by the average mortality of natural small pox would have died, if the constitutions of these forty-eight persons had not been modified by previous vaccination. Dr. Dewar, of Edinburgh, hearing that many vaccinated subjects had been affected with small pox at Cupar in Fife, where the natural small pox at the same time prevailed, he most laudably repaired to the spot to investigate the subject. He found that fifty-four vaccinated subjects had caught the small pox. All these, except one, had the mitigated or five-day eruptive fever and lived. The fatal case, was that of a child, who had a complication of other disorders, and having died on the fifth day, the small pox, according to its ordinary course of fatality, could not of itself be the cause of death. All the rest were safe; while of sixteen cases of the natural small pox at the same time and place, six died; so that if these fifty-three cases had not undergone the mitigating process of vaccination, nineteen or twenty would have perished. Between thirty and forty cases of the same kind have occurred at Carlisle, on the testimony of Dr. Barnes, a respectable practitioner of that city.† Many proofs might be adduced from the oral testimony

* See Inquiry into the Antivariolous power of Vaccination. Ed. 1809. There is an article in the Edinburgh Medical Journal by the same gentleman in 1819, in which he mentions that he had heard of several deaths having occurred from cases of small pox after vaccination. But, admitting this, it is utterly incomprehensible, by what process of reasoning Mr. Brown could, on such premises, arrive at the conclusion, that vaccination ought to be exploded and abandoned.

† See a clear and able exposition of this subject in the Medical and Surgical Journal of Edinburgh for July, 1818, by Mr. Dunning of Plymouth.

of private practitioners, which would over-swell this article. The only other to be mentioned is from the report of the Central Committee of Vaccination at Paris, made in December last, in which the description of the disease occurring after vaccination corresponds exactly with the mitigated five-day cases which have occurred in Britain. They refuse the name of small pox to it; but as I know from my own observation, as well as from the testimony of others, that the matter from it does by inoculation give the small pox, we can hardly perhaps with propriety deny it that name; but it should be distinguished by some strong discriminating epithet, such as is suggested above.

Now let all this be applied to the case of a community, in which the total eradication of small pox is quite hopeless. Let it be admitted that such occurrences as have been described do frequently occur: let it even be admitted, for argument's sake, that every vaccinated case whatever must of necessity and unavoidably, at some time or other in future life, be affected with this mitigated species of small pox, would it not even under this great abatement be one of the greatest boons that could be conferred on humanity, as an instrument or remedy which would disarm small pox of its danger? The next greatest benefit to the total extirpation of small pox, would be the stripping it of its terrors by rendering it safe and harmless.

It may be further remarked, that the benefit derivable from the different proportions of the persons vaccinated to the total population, advances in a considerably higher progression than the simple arithmetical. It is evident that the smaller the relative number of the vaccinated, the greater their chance of meeting with small pox infection; and that though the disease they may catch is of a mitigated nature, it would nevertheless be desirable to avoid it on its own account, but still more on account of the prejudice it creates. This, in the eye of general benevolence, constitutes an additional, though secondary motive, for extending the vaccine inoculation as widely as possible, even though the attainment of the *maximum* of total extirpation should be impracticable and hopeless.

It is of the highest importance to society that this subject

should be seen in its true light, and in all its bearings; for the frequent occurrence of these cases of small pox, however safe in themselves, have had a most pernicious effect on the credulous and ignorant, by giving a check to the practice of vaccination. How many parents are there now who, from a weak distrust in the virtue of vaccination, have to lament the loss of a child from small pox, either casual or inoculated? Many such are known to myself. It is pleasing, however, to observe, that though this unmerited discredit into which vaccination had fallen, swelled the number of deaths in London from small pox to 1051 in 1817, good sense is likely still to prevail; for last year (1818) the deaths have fallen lower than they have ever been known since the institution of the bills of mortality, the total number being only 421.

On the whole matter, I believe I am speaking the language of every man of good principles and feelings, capable of reflecting seriously and considerately on the subject, when I say, that whenever he applies his mind to it, he finds some new and increasing cause of complacency and satisfaction. Viewed as a mere physical fact in the natural history of the animal kingdom, the virtue of the vaccine *virus* in resisting the action of the *variolous*, is, by its novelty and singularity, highly striking and interesting to every one whose taste leads him to take delight in contemplating and exploring the devious ways and varied forms of nature, as curious exceptions to the uniformity and constancy of her laws. But the importance of this vanishes to nothing when the unexampled benefits of it to mankind are fairly weighed; benefits which could never have been dreamt of by the most sanguine philanthropist, who, in contemplating it, finds himself lost in astonishment, at a boon to mankind almost beyond the grasp of his mind duly to appreciate. It will in the eyes of future ages be deemed an *epocha* in the destinies of the world, and one of the highest boasts of the country in which it took its rise, with a sense of unrequitable obligation to the individual who first disclosed and promulgated the secret, by drawing it from the dark recesses of rural tradition, and rendering it available to the whole human race.

Such are the sentiments which must fill every well consti-

tuted mind, and it behoves the whole medical profession, which has already done itself so much honour by the zealous and disinterested encouragement afforded to it, to continue its efforts in eradicating every remaining prejudice against it. It becomes Englishmen, in particular, to foster it, not only as the native offspring of his country, of which he has reason to be proud, but to redeem the character of the nation from the reproach of having of all others, whether savage or civilized, done the least justice to this noble discovery. There is no country which has prized it less, nor availed itself of it so little. Have we not seen it adopted instantly in Peru, in consequence of a flash of conviction from the light of evidence, and have we not seen this conviction fully justified by the immediate disappearance of small pox from that whole region? To those nations who may feel an envy of the glory attached to our country by this discovery, it must be no small consolation to perceive, that a large proportion of the English nation has hitherto been so besotted as not to know how to appreciate nor to avail itself of it, and that it has encountered more opposition among ourselves than in all the world besides.

TABLE I.

TABLE I.

Years.	Total Mortality.	Mortality from Small Pox.	Proportion.	Proportion to 1000.
1706	22,097	1095	1 in 20	50
1707	21,600	1078	1 20	50
1708	21,291	1687	1 12½	79
1709	21,800	1024	1 21	49
1710	24,620	3138	1 8	127
1711	19,833	915	1 21½	46
1712	21,198	1943	1 11	92
1713	21,057	1614	1 13	77
1714	26,569	2810	1 9½	106
1715	22,232	1057	1 21	47
1716	24,456	2427	1 10	100
1717	23,446	2211	1 10½	94
1718	26,523	1884	1 14	71
1719	28,347	3229	1 8¾	114
1720	25,454	1440	1 17¾	56
Total,	350,503	27,552	1 12.7	78

In this series it appears that the deaths from small pox are, to the total mortality, as 1 in 12.7; that is, 78 in 1000.

TABLE II.

Years.	Total Mortality.	Mortality from Small Pox.	Proportion.	Proportion to 1000.
1745	21,296	1206	1 in 17¾	56
1746	28,157	3236	1 8¾	115
1747	25,494	1380	1 18½	54
1748	23,869	1789	1 13½	75
1749	25,516	2625	1 9¾	103
1750	23,727	1229	1 19¼	52
1751	21,028	998	1 21	48
1752	20,485	3538	1 5¾	172
1753	19,276	774	1 25	40
1754	22,696	2359	1 9½	104
1755	21,917	1988	1 11	91
1756	20,872	1608	1 13	77
1757	21,313	3296	1 6½	155
1758	17,576	1273	1 13¾	73
1759	19,604	2596	1 7½	132
Total,	332,826	29,895	1 11.2	89

In this series it appears that the proportion of deaths from small pox is, to the total mortality, as 1 in 11.2; that is, 89 in 1000.

TABLE III.

Years.	Total Mortality.	Mortality from Small Pox.	Proportion.	Proportion to 1000.
1784	20,454	1210	1 in 17	59
1785	18,919	1999	1 9½	106
1786	20,445	1210	1 17	59
1787	19,349	2418	1 8	125
1788	19,697	1101	1 17¾	56
1789	20,749	2077	1 10	100
1790	18,038	1617	1 11¼	89
1791	18,760	1747	1 10¾	93
1792	20,313	1568	1 13	77
1793	21,749	2382	1 9	11
1794	19,241	1913	1 10	99
1795	21,179	1040	1 20¼	49
1796	19,288	3548	1 54	18
1797	17,014	512	1 33½	30
1798	18,155	2237	1 8	123
Total,	293,850	26,579	1 11	90.9

In this series it appears that the proportion of deaths from small pox to the total mortality is 1 in 11, that is, 90.9 in 1000.

TABLE IV.

Years.	Total Mortality.	Mortality from Small Pox.	Proportion.	Proportion to 1000.
1804	17,038	622	1 in 27½	36
1805	17,565	1685	1 10½	96
1806	18,334	1297	1 14	71
1807	17,938	1158	1 15½	65
1808	19,964	1169	1 17¼	58
1809	16,680	1163	1 14¼	70
1810	19,893	1198	1 16½	60
1811	17,043	751	1 22¾	44
1812	18,295	1287	1 14¼	70
1813	17,322	898	1 19¼	52
1814	19,783	638	1 31	32
1815	19,560	725	1 27	37
1816	20,316	653	1 31¼	32
1817	19,968	1051	1 19	53
1818	19,705	421	1 47	21
Total,	279,404	14,716	1 18.9	53

In this series it appears that the proportion of deaths from small pox to the total mortality is 1 in 18.9, that is, 53 in 1000.

An Account of some Alarming Symptoms caused by Spiculæ of Bone in the Stomach. By JOSEPH M'SWEENY, M. D.

[From the Edinburgh Medical and Surgical Journal for January, 1820.]

ALTHOUGH the power of digestion has been found capable of subduing very hard substances, yet the human stomach, which often with difficulty digests solid articles of food, may be found sometimes incapable of acting on so solid a substance as bone. Even where the power of the organ is capable of acting on such a substance, in some cases so long a time may be necessary, that the foreign body may be productive of serious consequences. It is not to be inferred, that every substance difficult of digestion, or indigestible, is kept from passing out of the stomach. Daily experience shows, that the pylorus does not keep so strict a guard. Different indigestible substances have passed through the alimentary canal; even articles of food have passed unchanged. No doubt, pieces of bone of an unirritating shape have also passed without causing inconvenience. The pylorus not being irritated by a smooth body, can gradually dilate, and can allow it to pass without producing uneasiness; but where sharp spiculæ of bone have been swallowed unnoticed, alarming, perhaps fatal consequences have ensued, without the cause being suspected. From the consideration of the pylorus, we must perceive the obstacles it will oppose to the passage of a sharp body. It is endowed with contractile power, and is sensible to a stimulus. The contact of a sharp body must stimulate it violently, and must consequently cause it to contract, and thus oppose the passage of that which irritates it.

A gentleman at dinner swallowed a small fragment of bone. On the same evening, uneasy symptoms came on. He felt a pain in the centre of the scrobiculus cordis rather dull, but causing great anxiety. It sometimes changed its situation, but was mostly referred to the centre of the scrobiculus cordis. He sometimes had intervals of ease, at other times his sufferings became aggravated in a violent degree. He then could not point out the situation of the pain so exactly as before. It was felt more internally, as if some part inside was grasping

some sharp body. In this state his body was bent, his hand was firmly pressed against the region of the stomach, his pulse became small and quick, a cold sweat broke out over the surface, and his anxiety arose to an alarming degree. From this state he generally found relief by change of posture, and the pain mostly resumed its former situation in the centre of the scrobiculus cordis. He had a constant desire of keeping the hand on the situation of the pain. I had recourse to diluted muriatic acid as strong as I could venture to give it. All bad symptoms ceased under its use, and he found no inconvenience from the acid, which freed him from the state of suffering in which he had been. I met with similar symptoms in the case of a gentleman who was conscious of having accidentally swallowed a fragment of bone at dinner, a good many days before. I had recourse to a similar practice, and with complete success.

Mrs. P—le, of Nicolson Street, in Edinburgh, consulted me last winter about a dull kind of pain in the region of the stomach. Not being decided as to the nature of it, I merely recommended attention to regimen. She applied to me a second time, and stated that she was not better, and complained of the great anxiety it caused her. I now suspected, that the pain might arise from a spicula of bone accidentally swallowed. On close attention to the symptoms, I found they were the same as those stated above. She had the constant desire of keeping the hand on the situation of the pain, which was mostly felt in the centre of the scrobiculus cordis. There were intervals of ease; at other times the sensation of a sharp body being grasped, threw her into a state of agony. In this state, the pulse became small and quick, and a cold sweat broke out over the surface. Although she had not the least recollection of having taken any thing that would account for the symptoms, yet I did not hesitate in thinking, that the case was similar to those mentioned, particularly as she had taken food in which a spicula of bone might have been. I added as much acid to a half pint of water as I thought would be sufficient to relieve the symptoms without injuring the stomach. She took this quantity at one draught; but my disappointment was indeed great, when she complained of being rather worse

on the following day. The acid had evidently increased the irritability of the stomach without acting on the foreign body. I now supposed that the acid had passed out of the stomach without remaining a sufficient time to act on the spicula of bone; and as the irritability was increasing daily, I determined not to lose time, but to have recourse to the most vigorous practice at once. I made the diluted acid much stronger than what she had taken the day before; and having taken a quantity of it to be certain that it was not too strong to injure the stomach, I explained her case to her, and she willingly agreed to adopt any plan of treatment I should propose. She took a wine glass of this diluted acid every ten minutes. After a half pint of it was taken in this manner, she took the same quantity in one draught, and I was happy to find, after it, that the pain and anxiety had ceased, with which she had been in agony during thirteen days. She now said she felt only a general sense of soreness; she slept well that night; next day she still felt the general sense of soreness; but she was in good spirits, and completely free from the pain. Having recourse to the remedy quickly on account of the urgency of the symptoms, unfortunately I did not ascertain the strength of the diluted acid I used. The standard I employed was to try it on myself in every case, and to take it as strong as I could with safety. The plan employed to prevent the teeth from being injured, was, to make the patient open the mouth as wide as possible, and to protrude the tongue so as to cover the teeth of the lower jaw; the edge of the glass was then rested on the tongue, and the diluted acid was swallowed without allowing it to come in contact with the teeth of the upper jaw. The mouth was repeatedly washed with water. In this manner, the teeth escaped uninjured even in the case of the patient who had been affected thirteen days, and who took a great deal of the acid.

If this patient had been allowed to remain without medical assistance, it may be fairly presumed, that a fatal gastritis or ulceration of the stomach might have at length ensued. A thickened or ulcerated state of the pylorus might have been caused, and I am afraid, that affections of this kind have been classed under the title of scirrhous pylorus, while the cause of

the mischief has not been suspected. It is possible that an ulceration would take place, through which the contents of the stomach would be extravasated into the abdomen. A small fragment of bone might easily escape observation when enveloped in the ingesta of the stomach. It would be a desirable thing that the attention of examiners would be in future directed in search of such a cause, in cases of extravasation of the contents of the stomach into the abdomen, and in cases of diseased pylorus. There is some danger of confounding the symptoms with those of cramp of the stomach, as a patient may have swallowed a spicula of bone without being aware of it. The bent position of the patient, the contortions of his body, his hand pressed against the seat of the pain, the anxiety depicted on the countenance, and the intervals of ease, may deceive a practitioner suddenly called in, and may lead to injurious practice. These symptoms may be distinguished from those of the cramp of the stomach, by the following marks: The pain is mostly felt in the centre of the scrobiculus cordis, and is described as if it could be covered with the point of the finger. In this situation, although the patient has a desire of pressing the hand gently against the seat of the pain, yet very firm pressure increases it. When the pain is felt more internally, causing the sensation of something being grasped, firm pressure gives relief, the pulse becomes quick and small, and a cold sweat breaks out over the surface. Perhaps it may be asked, would not it be better to employ an emetic in such a case? A person took an emetic on account of having accidentally swallowed a sharp body, and soon after expired. As the surgeon who was called in to examine the body, and by whom I heard the case related, will probably give the particulars in some periodical work, I shall not anticipate him; but the account of the dissection was quite sufficient to prevent me from employing an emetic. As a sharp body is prevented from passing, on account of its irritating the pylorus, a purgative medicine was, I think, contra-indicated; it would be likely to make the pylorus suffer more. The diluted acid must have afforded relief by acting on the sharp irritating points which were fortunately most exposed to its influence, and thus allowing that to pass

which was before prevented by the irritating points causing the pylorus to contract violently, when they came in contact with it.

Cases of Severe Dyspnœa in Children, connected with a Singular Affection of the Throat. By John Abercrombie, M. D.

[From the Edinburgh Medical and Surgical Journal, for April, 1819.]

I have met with three cases of violent difficulty of breathing in children, connected with an affection of the throat which was new to me, and perhaps may be not unworthy of a corner in your journal.

Case I.—Was a child about two years of age. I found him affected with severe difficulty of breathing, resembling the last stage of croup, inspiration being particularly laborious, and performed with a strong exertion of all the muscles connected with it, and a sound which conveyed the impression of great interruption to the passage of air through the larynx. At first sight I considered the disease as croup; but on enquiring into the history of it, I was told that the affection had existed in nearly the same degree of violence for eight days; and that the health of the child had not been in other respects remarkably impaired. Concluding from these circumstances that the disease could not be croup, I was led to a particular examination of the throat, where I found the tonsils and other superior parts quite healthy; but in the lower and posterior part of the pharynx I observed an appearance of fulness, which, however, was situated too far down to be seen distinctly. On examining this part with the finger, I discovered a tumour, arising from the posterior part of the pharynx, and filling up the space completely, so as to press considerably upon the larynx;—the tumour was soft and fluctuating, and evidently contained a fluid. With considerable difficulty I succeeded in puncturing it, by directing a lancet along the fore finger of the left hand, which was kept in contact with the tumour, and purulent matter was discharged from it, with immediate and complete relief of all the symptoms. In the course of the fol-

lowing night, however, the dyspnœa returned; and on examining the throat next day, I found that the tumour was again filled with matter. It was therefore necessary to puncture it again, by which the dyspnœa was again completely relieved. In this manner it required to be punctured four or five times, after which the case terminated favourably.

Case II.—Was a child aged eighteen months, who had passed through measles eight or ten days before I saw her. Her breathing had first become difficult during the eruption of measles, and after partial relief, procured by the usual treatment, had again become difficult after the decline of the eruption. The difficulty had been gradually increasing, particularly during the last three days, and in the night before I saw her, had been so great as frequently to threaten immediate suffocation. On examining the throat, I discovered a tumour which exactly resembled that described in the former case, except that it was situated rather lower down, so that it could only be felt by the finger, and nothing remarkable could be detected by looking into the throat. With some difficulty the tumour was punctured by means of a small trocar, such as is used for hydrocele, the canula being first carried down so as to bring the point of it into contact with the tumour, and then the stilette introduced through it. There was a copious discharge of purulent matter, with immediate relief of the symptoms; and I requested an intelligent young medical man, who was related to the family, to make pressure on the tumour several times in the day, so as to prevent it from filling again, as in the former case. By this precaution there was no return of the symptoms. A remarkable circumstance in this case was, that the child appeared to have little or no difficulty in swallowing.

Case III.—Was so exactly similar to the preceding, that it is unnecessary to describe it particularly. The tumour required to be punctured a second time, after which the case terminated favourably.

Though not immediately connected with that subject, I take the opportunity of adding a very few observations on the larynx as a seat of disease. The more violent diseases of this organ have been investigated with much attention; but I think there are more obscure affections of it, which present a most

important field for observation, in which, hitherto, little has been done. Many obstinate cases of *cough* probably have their seat in the larynx, which we are sometimes disposed to refer to the lungs. I lately saw a gentleman, about fifty years of age, who was affected with the most violent cough I ever met with. It occurred with greatest violence in the night time, when it frequently kept him for an hour or two at a time in one continued paroxysm of the most violent convulsive coughing; his features became turgid; he nearly lost his recollection; and there seemed every reason to fear that the paroxysm would terminate either by suffocation or apoplexy. The disease had continued about a fortnight, and the most active remedies, large and repeated blood-letting, blistering, opiates, and antispasmodics, had not made the slightest impression upon it. His pulse and breathing were natural; he had a peculiar hoarseness or huskiness of his voice; and, upon inquiry, he mentioned that the paroxysms were preceded by a particular sense of irritation about the larynx. The application of six leeches to the region of the larynx broke the force of this frightful disease, and a second application of the same remedy completely removed it.

It may be worthy of investigation, whether hooping-cough has any relation to the larynx. Some cases also, which have been considered as consumptive, have afterwards turned out to be connected with disease in that organ; and, upon the whole, there seems to be much room for extensive observation, with the view of introducing precision and accuracy into the pathology of cough.

Observations and Experiments on the Nature of the Morbid Poison called Yaws. By James Thomson, M. D., &c.

[From the Edinburgh Medical and Surgical Journal, for July, 1819.]

The general appearance of the eruption in this disease has been accurately described by every writer on the subject, but many particulars necessary to an accurate knowledge of the

laws that it follows have been entirely omitted, or rather never ascertained. Many British practitioners have exclaimed against this neglect, and the more so, as the disease is of so common occurrence in many of the West India islands, that it comes within every one's observation. To a person who has resided any time in the West Indies, the cause of such neglect is easily explained. There is a sort of actual horror on the countenance of most practitioners at the sight of a patient covered with the eruption. He views him afar off, and his opinion is given in the most hurried manner. The dreadful stories of white people having been infected, the shame attached to it, and, more than all, the fear of his prospects in life being blasted, are sufficient to counteract every desire of advancing his knowledge of the disease. The consequence of this is, that the yaw patients are banished to some remote corner of the estate, where they are put under the care of some old woman, who keeps the sores clean, and conducts them through the disease. If, from neglect, symptoms called the dregs of the yaws should appear, or if any hereditary tendency should have been called into action, the practitioner may again see the patient, but he has only one mode of practice, an alterative course of mercury, and *that* the patient *must* undergo. If the symptoms should cease, in a short time they again return,—a second course must be submitted to, for the first was not sufficient. I am ashamed to mention the number of such courses I have seen a poor creature go through, and I hardly would get credit if I related what has been told me by old practitioners. Such symptoms, I shall afterwards show, are, in general, incurable; all we can promise is to mitigate them with a generous diet, and the use of the woods employed by the natives. One would have thought that their success might have taught medical men to follow their plan; but no, there is a charm in an alterative course that has sent thousands to their graves. Happily for mankind, the charm is fast losing its power, and in a few years practitioners will ridicule our notions of its virtues. A perusal of the excellent production of Dr. Adams on morbid poisons first induced me to turn my attention to this subject, and I trust he will find some of his queries answered in this paper; and nothing could give me greater pleasure than to

satisfy the doubts of a physician so eminently qualified to extend our knowledge by his accurate mode of observation.

This disease is constantly preceded by symptoms of languor and pain, amounting in most cases to fever, which is more severe in children. Before the eruption appears, I have frequently seen the whole surface covered with a white scurf. It looks as if the skin was dusted with flour. It remains only a few days, and then small pimples may be seen on the forehead and other parts of the body. These continue to increase for six or ten days, when they get a crust on the top, and ill formed pus can be detected. The pustules go on increasing in size, covered with loose irregular crusts; many of them acquire the size of a shilling. If you remove this crust, you find a foul sloughy ulcer beneath. I have endeavoured to ascertain the exact period when you can find the fungus to shoot up, but it is irregular. I have found it so early as one month from the eruption, and so late as three. We cannot, therefore, take this as the criterion of the second stage of the disease. It seemed to depend much on the constitution of the patient, and appeared sooner on those that were well fed. A second attack of fever will occasionally bring out a new eruption. These proceed in the same way, so that we have sores in all states on the body at the same time. When the disease attacks the throat, it is extremely distressing; the ulcers resemble a piece of *toasted cheese;* they never have any fungus; and before the susceptibility is destroyed, the patient is deprived of the greater part of the palate.

The yaw shows a difference according to the subject. In healthy patients it is red, like a piece of flesh; when weakly or diseased, it is white like a piece of cauliflower, and bleeds on the slightest touch, and is by no means so elevated. In this state it remains for some time, till the fungus gradually contracts, diminishes in height, and is finally covered with skin. The yaw leaves no mark in general, except in those places where the inflammation has run very high; the scar in that case resembles the cow-pox scar, only it is broader, and more superficial.

The duration of the disease is various. Some will get rid of it in six months, others not till twelve; the average may be

stated at eight. The violence of the eruption and fever does not seem to depend, as in other exanthemata, on the peculiar susceptibility of the patient, but merely on his bodily strength at the commencement of the disease: for in weakly ill fed children it is uniformly more severe. The hairs are represented by many authors to be turned white. This I am confident is erroneous, and proceeds only from want of cleanliness. In all cases where proper ablution is used, no such thing can be observed. Where the hair was destroyed, it seldom ever returned, and then it was of the natural colour. The disease is classed among the tuberculæ in the arrangement of Willan. What right it has to be placed there I cannot imagine; for surely a tubercle and a fungus are quite distinct, and the fungus comes on at a late period of the disease. The eruption is at first papular, then pustular, and after that rises the yaw, but this is not constant; the ulcer may heal without any rising. If, therefore, we must have it to conform to an artificial arrangement, it must be classed among the papulæ, then transferred to the pustulæ, and afterwards carried forward to the tuberculæ; but if no fungus shoots up, it must remain as a pustular disease; or, if we will be still more scientifically minute, those sores that have a fungus must be called tubercular, and those that have not, pustular, even though they are within a few inches of each other. Nosology has already led to more absurd conclusions, and it will still continue to involve those who patronize it in more ridiculous difficulties.

The first question I endeavoured to ascertain was, the *latent period*. Chance first informed me of this. A number of healthy negroes were removed from a mountainous situation to a sugar estate. The children were mixed with those already on the property, and had their meals together. Seven weeks after their intercourse, three were seized with fever and pains; the eruption appeared all over the body; the rest, at the end of ten weeks, showed symptoms of the disease, and in eight months they had all recovered. Not satisfied with this accidental experiment, I was allowed to inoculate a child with matter. I ordered it to be taken from ulcers where the scab had been removed, and inserted in five different places. Three of the punctures healed, the other two did not heal for three

weeks; they resembled a scratch that proves irritable. From that time small sores appeared, which increased until they became foul, sloughy, ragged ulcers. Seven weeks from the time of insertion, pimples appeared on the forehead, and over the body, the fungus shot up, and the patient had a copious eruption, and did not get clear of the disease before nine months; the sores had no particular appearance. I may remark, that the two ulcers on the arm never showed any fungus, and left deep scars.

The small-pox having broke out in a mountainous situation, recourse was had to inoculation. Matter was taken from an apparently healthy negro girl, who had already been inoculated for the small-pox. This matter was inserted into the arm of only one subject. When the negro girl knew this, she came in great anxiety and confessed she had the yaws. It could not be believed, for her skin was shining and quite healthy, but on lifting up the arm, three sores were found in the axilla. The patient that had been inoculated had the small-pox mildly. At the end of two months the yaws appeared, and were neither milder nor shorter in their course than usual. The puncture made where the small-pox was inserted healed up in the proper time, and the secondary pustules were all dried up long before yaws appeared. This case might lead us to conclude, that inoculating with the circulating fluid of a yaws patient will produce the disease; but I have since found, that it is not the case, and that there must have been some undiscovered cause of fallacy in the example of the negro girl. I ordered four children to be inoculated, each in five places, with blood taken from a subject covered with yaws. The punctures healed immediately. In this fact, the yaws follows the course of the facts known with regard to other morbid poisons. The case of yaws mentioned by Dr. Adams as occurring in a Danish nobleman who had been *ten* months absent from the West Indies, is at variance with the facts just stated. I will not venture to reconcile them. I may merely state, that several old practitioners, to whom I have mentioned the fact, were of opinion, that the latent period could never have been so long. These cases also go to prove that inoculation does not render the course of disease either shorter or milder, and many other considerations confirm me in this opinion.

The patient is liable to other exanthemata during the yaws. I have known them to have small-pox and chicken-pox. I ordered several yaws children to be vaccinated. The cow-pox went regularly through its different stages; there was no observable difference in the time of healing in the sores. The matter was not taken in these cases. I did not feel myself warranted in doing so.

Matter was taken from an active yaws sore, and inserted into the axilla of a rabbit and a dog. No effect followed. It was repeated twice, but the punctures always healed. Matter was made up into a bolus, and given to a dog and a fowl, with no effect. There is an eruptive disease in cattle that the negroes call the yaws, but there is no analogy between them.

This disease can only be communicated once; the susceptibility is destroyed; but there frequently happens very unpleasant symptoms to a person who has undergone the eruption. For example, a mother who gives suck to a child with yaws, has the nipple excoriated from the acrid matter around the mouth; the irritation spreads, and a foul ulcer consumes the breast, and produces death by spreading to the axilla; the child also perishes, unless supported by the greatest care, for it is abandoned by every person.

There is a singular fact with regard to the history of the yaws not generally known, but not the less certain. If a person with a large ulcer contract the yaws, he may pass through the disease without any eruption. At a certain time, the sore begins to acquire a new granulated appearance, and becomes elevated, the edges of the ulcer become characteristic, and a constant discharge is kept up. If this sore is attempted to be healed, and we succeed, an eruption of yaws follows in the regular manner. Otherwise, it goes on, and generally remains as an ulcer for life. The practice always to be pursued is, to endeavour to heal the sore the moment we suspect any infection.

I have endeavoured, by every means, to ascertain whether the disease can be communicated by the mother to the child *in utero*, but without obtaining any satisfactory information. Many old negro women have told me, that a child born under these circumstances never has the yaws, but no reliance can be placed on their testimony. Equally suspicious is any infor-

mation I have on the question, whether the air can propagate the disease. The subject is so open to fallacy, that I would place reliance on no conclusion. To determine at what time the eruption becomes infectious, and when it ceases to be so, would require more facts than what I am yet in possession of.

The practice now generally adopted in this island, is to leave the disease to the efforts of nature, to feed the subject with good diet, and do light work. In weakly children, towards the conclusion of the disease, the decoction of woods, flowers of sulphur, or antimonial preparations, are of considerable service.

Any proprietor of negroes is well aware of the loss he sustains from the yaws, not so much from the actual disease, as from the fatal symptoms that too often follow. The finest looking slave will, in consequence of such effects, in a few months become a burden to himself and his master. The yaws have their bitter dregs, as they are vulgarly called, as well as other diseases.

If the patient has been ill fed or caught cold, and the eruption repelled, he generally dies dropsical. When the disease has attacked the throat, the soft parts are always lost; repeated inflammations ensue; and, from being considered by the ignorant as venereal, the poor creature, to his present misfortunes, has that of a needless salivation added. Mercury is of trifling service. A regulated diet, and the use of the country remedies, is much more efficacious; but the most unpleasant consequence is, that, if there should be any tendency in the constitution to hereditary disease, it is universally excited into action. The patient, shortly after the eruption ceases, begins to complain of pain and stiffness of all the joints. He loses flesh, the bones swell, the voice becomes guttural, there are patches over all his skin, the feet swell, and repeated attacks cause them to remain so permanently; the bones are swelled and deformed, and, finally, some ulcer breaks out, and the patient is rendered an object for life. These, and many others, are called the effects of ill cured yaws, but I am convinced, with little foundation. Symptoms of a similar nature begin to show themselves in many negro children at or before the age of puberty; previously, we could not wish to see finer live-

lier creatures. They begin to pine away, worms are suspected, and remedies are administered without relief. A sore at last may break out, and the symptoms may here remain stationary, but more generally the disease goes on, and the patient becomes disgusting to every one. He lingers a few years in some lonely spot. By most people in this country these symptoms are called venereal, arising from *rotten* parents, as they are termed. My views of the venereal disease would never allow me to agree to such a doctrine; and, after giving the subject every attention, I am convinced we must look for their origin at a different source. They will be found to be leprous affections in varied forms, arising from the hereditary tendency that is known to exist in such diseases. The confusion that prevails in the history of leprosy, shows sufficiently the different forms that climate imposes upon it. The negroes were formerly very subject to it. From better living and other causes, the tendency has not been roused except in some particular instances; and that severe form of it mentioned by many authors is hardly to be met with.

I am convinced that such cases are not venereal, because such severe cases are not known in this climate. The children at birth are healthy, and remain so many years; the parents are found often to have no disease, but a leprous affection can be traced to some one more remote; the symptoms are not curable by mercury, and it only attacks particular children of a family. The leprous children who derive the disposition from their parents, escape the disease till near puberty. Climate has varied the forms of the leprosy, witness the *coca bea*, or joint-evil. It has many of the characteristics of true leprosy; and has not climate so modified the virulence of the venereal disease, as to enable the constitution to overcome it without the aid of mercury?

I have often been astonished that mulatto children, generally born of healthy parents, are more subject to these symptoms than black children. I have often suspected, that, in the crossing of the different races of beings, such hereditary tendencies were more liable to be excited. I lately found this conjecture to be correct, from the information of an intelligent gentleman who resided many years in a Spanish colony

where leprous affections were very prevalent. He said that the natives have an old idea, that, if a man have connection with a black woman predisposed to leprosy from her family, the offspring will become diseased. This they prove by citing many cases of negro women whose children, by white men, were diseased, but whose future offspring, by husbands of their own colour, have remained perfectly healthy; and they attribute the exemption of the higher orders in society from these diseases merely to their avoiding the exciting causes.

Whether these remarks on leprosy are correct, others having more extensive opportunities must judge. My chief object in making them is to do away with the abominable practice of the indiscriminate use of mercury. It ultimately ruins the individual, while a more simple treatment would render his life comfortable.

The yaws has been compared with that disease mentioned by Moses in the 13th chapter of Leviticus. If medical men can reconcile them, it is much to their credit. In my humble opinion, that chapter requires a very ingenious commentator to explain it.

I have by me the notes of an old practitioner of this island, who went home to a part of Scotland many years ago, where the *sibbens* was prevalent. His remarks are made without any allusion to theory, and they tend to confirm the identity of yaws and sibbens in many particulars. Observations from a person who had seen both diseases are peculiarly valuable. It would be improper to lengthen this communication with entering on this point at present. I shall take another opportunity of offering you some farther researches connected with this subject.

Case of Survival of Twelve Days' Abstinence from Food, occurring in a Person in the state of Health. By Thomas T. Griffith, Member of the Royal College of Surgeons.

[From the London Medical and Physical Journal, for February, 1820.]

The leading particulars of the following extraordinary case, having happened in part under my own immediate observation,

and in part under such circumstances as leave but little room to doubt their having occurred, I am induced to request a place for the insertion of them in your Journal.

On the morning of the 27th of last September, an immense body of water, which had accumulated in one coal-pit, suddenly burst into another, where nineteen men were at work; sixteen of them were enabled to extricate themselves from the perilous situation which they were thus unexpectedly placed in, at a distance from the surface of one hundred and twenty yards. But, such was the quantity of water, and the changes it rapidly produced in the pit, that the remaining three were cut off from all means of escape. In the hope of being able to rescue these unfortunate people, every measure that could possibly expedite the discovery of them was unceasingly pursued. On the ninth day the bodies of two, who had been drowned, were discovered, and on the twelfth day some vestiges of the third man were traced; in a short time his voice was heard; and soon afterwards he presented himself to the astonished workmen.

It appeared from his subsequent statement, that, when the water was rushing in an overwhelming stream towards the spot where he stood, he had only sufficient time to allow his gaining a situation above the highest level to which the water reached. In this place, which was a dark, low, close passage, communicating with two galleries, he remained for *twelve days*, without any other sustenance than a little water, which, trickling down a rock distant about twelve paces from the place where he lay, he collected in a hollow formed by his hand; to this he occasionally crept on his hands and knees, and at other times he relieved himself by merely changing his posture. He computed the period of his confinement to have been about *nine* days. For the first third part of this time he felt a considerable desire for food, which afterwards entirely ceased; and he only experienced an occasional painful sensation in the region of his stomach, which was always removed by drinking a little water. The greatest part of the time was spent in sleep, accompanied by dreams. Some of the functions of the body appear to have been nearly suspended, and others to have been very slowly performed. The bowels were reliev-

ed once only; there was no sensible perspiration; the breathing was slow; but the urine was voided in small quantities about twenty times. His mind suffered but little depression, except upon one point; in general, it was at ease, and he felt confident that at the end of a computed time the water would be removed, and a communication with himself be opened: but, when he reflected upon the destitute and unhappy state of his wife and children, the powers of reason yielding to a pleasing hallucination: he thought that celestial sounds were near him; every unpleasant feeling ceased, and happier and more animating views employed his thoughts.

At the moment he was found, and for some time afterwards, he declined taking any thing more than a little dry biscuit; he was therefore merely removed to a warmer part of the pit, where I first saw him. He was in full possession of his mental faculties, his spirits were buoyant, and his feelings alive to the hearty congratulations of his companions, and to the anticipated pleasure of returning to his family. His face and figure were much emaciated;* there was an expression of wildness and famine in his countenance; his cheeks were flushed; his tongue was slightly furred and white; his pulse was regular, about 112, but very weak and small; and his extremities were cold. He complained of some degree of thirst, but of no pain: he had no wish to take food.

Fearful of calling into sudden or violent action the accumulated irritability of the system, liquid food, of a mild nutritive kind, was administered to him in small quantities, and at stated intervals: although he took it with indifference, his stomach retained it. The circumstance of his deliverance, and his anxiety to see his family, excited his mind so powerfully, as to give an unexpected degree of tone and energy to his whole frame; of this I thought it right to avail ourselves, and to remove him from his present situation to some house, where we could better guard against the effects of that debility which might be expected speedily to result from his present state.

* On the fourth day from that on which he was found, he weighed twenty-eight pounds less than he had done some weeks previously. He is a muscular bony man, of the middle size.

He was brought up from the pit in a tub, so large as to admit of his lying down in it, lest a continuance in the erect posture might have induced syncope, under circumstances very unfavourable to his recovery. In this way he experienced very little inconvenience from his removal. During that evening, and the following night, he was ordered to take the yolk of egg, mixed with warm water and sugar, weak broths, gruel, milk, &c. in larger quantities and with longer intervals; and, once in three hours, part of a slightly-cordial mixture, containing a few drops of laudanum in each dose. The nurse was directed to wake him occasionally, if much disposed to sleep.

Sunday, (the following morning.)—He has passed a tolerably comfortable night, occasionally slumbering. He has taken some quantity of food without any sickness. He now feels much exhausted; his pulse is languid, and reduced from 112 to 60 strokes in a minute. He has no desire for food: faintness comes on in the erect posture. He has voided urine. A more nourishing and stimulating diet was ordered. In the evening he was better, but suffered some degree of uneasiness from flatulency and heat. As his bowels had not been relieved for some days, the following mixture was prescribed: ℞ Pulv. Rhei ʒij. Potassæ Tartr. ʒj. Tinct. Sennæ ʒij. Aquæ Puræ Infus. Rhei āā ℥iss. M. Capiat cochlearia ij. majora 3ta. quâque horâ, donec alvus respondeat. The operation of the medicine produced four evacuations of hardened black fæces, and afforded the patient much relief.

Monday.—He is better; has passed an easy night with some sleep. His pulse is 84, and fuller. His appetite is returning. He complains of an unpleasant degree of heat in his feet. The same kind of diet is to be continued.

Tuesday.—This morning I was requested to visit him as soon as possible, as he was much worse. I was informed, that, finding himself better on the preceding evening, and his appetite greater, he had taken solid food, and ale in small quantities: in consequence of which he had had a very restless night; his skin had been hot and dry; his bowels distended and painful; his stomach oppressed; he had been thirsty, and at times delirious. Nor did these symptoms disappear, until the stomach had been relieved of its contents, and the bowels

had been acted upon by an aperient. Two or three days of restlessness, heat and languor, with diminished appetite, elapsed, before he recovered that point of convalescence which he had thus lost by his own temerity and imprudence. Such a degree of reaction took place in his feet and toes, as seemed for a short time to threaten the vitality of those parts.

During the ten or twelve ensuing days, a variety of symptoms, chiefly arising from the different states of the alimentary canal, appeared. For a day or two the pulse intermitted, but was restored to its regular action by emptying the bowels. Nothing material, however, prevented his ultimate recovery; and his general health is now, December 16th, perfectly re-established. His last complaint was of a severe rheumatic pain in the shoulder, on which he had lain during the greater part of the period of his confinement in the pit, and which he still continues to suffer at times.

Remarks.—It may be asked, "were there any peculiar or local circumstances which could have aided the powers of the system in supporting life, for so long a period, under a deprivation of food, so entire?" I believe that, providentially, there were. The man's age 27; his bodily strength, his habits, and that self-possession and strength of mind which led him to an almost correct calculation of the time required for the removal of the water and other obstacles, and enabled him to wait patiently the expiration of that time, in a confident hope of being ultimately rescued, were advantages greatly in his favour. But there existed also a local cause,—the peculiar kind of air which he breathed; and which, I am disposed to think, contributed materially to the preservation of life. From repeated examinations of the air in the pit, from my own feelings and observations whilst below, and from the man's description, it was ascertained that, with the water, a quantity of carbonic acid gas had diffused itself through the lower parts of the pit. The situation which the man occupied was an elevated one, and would therefore raise him above the more concentrated stratum of carbonic acid gas; but he was still aware that the surrounding air was heavy and oppressive: he said, "that for the greater part of the time he breathed the *damp* air, but that it became better as the water lowered." To enter again upon

the particulars of the case, or to reason upon the known properties of carbonic acid gas, and its effects under different modifications, would extend these observations beyond their proper limits. From the positive evidence of the man having breathed an atmosphere containing a greater or less proportion of carbonic acid gas, for twelve days, and from his having survived, may we not infer, that this gas, by producing a sedative effect on the system, by moderating irritability, and restraining action, prevented the active exercise, and consequent exhaustion of the vital powers, and thus proved conducive to the preservation of life?

Experiments on a few controverted points respecting the Physiology of Generation. By James Blundell, M. D. Lecturer, in conjunction with Dr. Haighton, on Physiology and Midwifery, at Guy's Hospital.

[From the Medico-Chirurgical Transactions, Volume X.]

Among the various questions which have been raised respecting the generation of animals, there is one, as yet undecided, which has not perhaps been hitherto investigated with all the care it deserves. It may be demonstrated by experiment, that, in this curious process, the male furnishes the semen, and the female the rudiments; but whether these two substances must have access to each other, in order that the young animal may be formed, is a question which still admits of dispute. It is true, indeed, that many naturalists have *asserted*, that contact is necessary; and Spallanzani has even gone so far as to demonstrate that it certainly takes place in the generation of the frog and toad. Still, however, notwithstanding the labours of physiologists hitherto, we are not, I believe, as yet in possession of any regular system of experiments, which proves that the semen must have access to the rudiments, in those forms of brute generation which most nearly resemble our own. In the present state of our knowledge, the reverse of this position seems, at least, not improbable, as the experiments of Dr. Haighton, a valued relative

of mine, have shown, that evidences of generation may be produced in the ovaries, although the semen has been excluded previously to sexual intercourse by the closure of the fallopian tube.

The principal object of the memoir, which I have now the honour of presenting to the Medico-Chirurgical Society, is to contribute some little towards the supply of this defect. In it I have endeavoured to show, that the semen must have access to the rudiments, in order that the young animal may be produced; and yet, that generation, although these approaches are necessary for its completion, may, to a certain extent, be accomplished without them.

As the rabbit was the animal, on account of its natural aptitudes, selected for my experiments, it may be proper, perhaps, before I enter on the recital of them, to premise a few remarks on its genital system.* In the fallopian tubes, and ovaries, and, I may add, the *external* genitals of the doe, there is little, when we view the organs as they are suspended in the glass, to attract the attention of the observer. It is different, however, with the vagina and the wombs; these are so strongly contrasted with the corresponding parts of the human organs, the wombs, by their tubular form, and the vagina by its length, its laxity, and large diameter, that they cannot be overlooked.

The vagina, when full grown, is about four inches long, and so capacious that, without much stretching, it will readily admit the extremity of the fore-finger. Its size, indeed, is so considerable, that it makes an approach to that of the human vagina, and greatly exceeds the dimensions of the same canal in a moderate-sized monkey, preserved in the obstetric Museum, at Guy's Hospital.

The wombs, the structure of which is scarcely less remarkable than that of the vagina, are two tubular organs, when unimpregnated, about three inches and a half long and about two lines and a half in their diameter; they are therefore, it is obvious, very unlike the human uterus, and rather resemble that

* It is scarcely necessary to remark that this description is not addressed to those who have made a study of comparative anatomy.

of several of our domesticated animals, as the cat, for instance, the bitch, and the females of the rat and mouse tribe. These two wombs, it should be further remarked, communicate with the vagina by two distinct orifices; and they are so completely independent of each other, that the one may be removed, without injury to the other, excepting a slight and superficial wound of that part where their necks lie in contact, and cohere.

Both the wombs and the vagina are, in these animals, furnished with longitudinal and annular fibres of a muscular structure, similar in kind to those of the intestines, but grosser and more distinct. In addition to these, along the inner margin of the wombs, from one extremity to the other, there runs a broad strip of fleshy fibres, which may, perhaps, not improperly be denominated the mesometric. I give the muscle this name, because it covers no inconsiderable portion of what may be called the *mesometry;** a delicate double membrane, the production of the peritoneum, which performing, for the tubular wombs, the office of a mesentery, unites them, like the intestines, to the chine. It is allied to the broad ligaments of the human womb.

All these fleshy fibres are animated with a very lively irritability. The mesometric muscle† changes the situation of the wombs. The wombs themselves perform a peristaltic action. The vagina not only performs this action, but an additional movement, which I shall hereafter have occasion to describe.

Such are the most striking characteristics of the genital system in the rabbit, those, at least, which the following experiments require me to notice. I may now proceed to the experiments themselves.

The first set of experiments was instituted with a view of ascertaining whether the semen and rudiments must have access to each other, in order that the young animal may be formed. For this purpose, an incision was made into the cavity of the belly, immediately above the wombs; and these,

* I venture on the name with diffidence, but no preferable term occurs to me; its etymology is obvious, and I believe, legitimate and analogical.

† Is this muscle allied in function to the round ligaments of the human womb?

together with the upper part of the vagina, were pushed through the opening. One of the wombs was then divided near its mouth, in a transverse direction, just as a piece of intestine might be, so as to separate it into two portions, the superior and inferior; or, as they may be designated from the annexed parts, the vaginal and fallopian. After this division the organs were immediately replaced, and the wound was sewed up.

Notwithstanding this violence, in the course of a few days, or a few weeks at farthest, most of the rabbits recovered their health, and at different intervals became fit for the approaches of the male. But though the general health was restored, the recovery was not complete. The operation, as subsequent dissection proved, had the effect of interrupting the canal of the womb, its tubular cavity growing up at the line of division, so that the communication between the vaginal and fallopian pieces became intercepted, and the semen and the rudiments could have no access to each other.

In this condition of the genitals, as soon as the sexual ardour was rekindled, the animals were submitted to the male; and, excepting in one or two anomalous instances, out of ten or twelve experiments, they all became pregnant from the *first* admissions. At different periods from impregnation the sexual organs were examined after death with great care and deliberation, when young animals were invariably found in the sound womb, but none in the interrupted. This, it is true, like the human uterus in extra-uterine pregnancy, was in many instances enlarged and developed and plentifully supplied with blood: indeed it often appeared as well adapted as its fellow for receiving and cherishing the rudiments; but with all its aptitudes for generation, it lay under one capital defect, its canal was interrupted; it intercepted the access of the semen to the rudiments, and without this access, generation could not be accomplished.

To confirm this conclusion, the accuracy of which I doubted at the time, it was determined to submit it to the test of another train of experiments. In these it was my object, to preserve the principle of the preceding operation, the exclusion of the semen from the rudiments; and yet at the same

time, to vary its circumstances as much as possible, in order to ascertain how far they had affected the result; for I need not observe, that circumstances often exert a silent and most fallacious influence over our experiments, our negative experiments especially, to be deprecated the more, because, from its insidious nature, it is so frequently overlooked.

In this second series of experiments, therefore, instead of operating upon rabbits that were full grown, I made use of those only that were under their puberty; and instead of interrupting, as before, the canal of the *uterus*, I interrupted that of the *vagina*.

The vagina of the doe, it has been already observed, is at least three inches in length; so that although it is interrupted at the uterine extremity, there still remains sufficient room for the male organ. Of this peculiarity I availed myself, in conducting these experiments; and instead of cutting the uterus, I cut the vagina asunder, near to the mouth of the womb, so as completely to interrupt its canal. In other respects the experiment was conducted as before.

This operation proved dangerous, much more so than the former; a number of the rabbits however recovered, and admitted, without repugnance, the approaches of the male. The result was decisive. Although the external genitals of these animals were turgid with blood, and the sexual excitement of some was remarkably lively; although too, in some of them, intercourse was renewed at intervals of a week or a fortnight, on the whole, as many as twenty or thirty times, not one became pregnant. Desire itself, in one or two instances, seemed almost insatiable; and in the rest, though suspended by coition for a time, in the course of a few hours, or a few days at farthest, it invariably recurred.

The same general appearances were observed on dissection, in them all. The vagina, if the operation had been properly performed, was completely interrupted. In both the ovaries there were *corpora lutea*. In some cases, the wombs appeared to have undergone little change; in others, they were enlarged, and evolved as completely as in actual pregnancy; but in no one instance was there the appearance of a single *ovum*, extra-uterine, or in the womb. In these, as in the preceding ex-

periments, though in a different manner, the access of the semen to the rudiments had been intercepted, and under these circumstances, notwithstanding repeated commerce with the male, the formation of the young animal could not be accomplished.

In performing the experiments recorded in the preceding paragraphs, there are various little niceties in the mode of operating, the observance of which is necessary to ensure success. The incision which is carried through the abdominal coverings, may be made in the linea alba, and should be eight or ten lines, at least, in length, in order that the parts may be replaced with facility. It should, too, lie as close to the *symphysis pubis* as possible, that the intestines, which in this herbivorous animal are numerous and cumbersome, may not, as they are apt to do when the incision is higher, protrude at the opening. It is true, indeed, that if the incision is placed in the vicinity of the pubes, the bladder, when it is distended, will fall in the way; but if the operator possess the requisite dexterity, there is no danger of wounding it; and a gentle pressure, persevered in for a time, will occasion it to withdraw into the pelvis. It deserves remark, however, that to produce this contraction, a little perseverance is necessary; for the bladder is not, in this manner, so readily excited to contract, as from previous reasonings on its irritability, we might have been led to expect.

To close the abdominal opening, the glover's suture will serve as well as any other; nor does the including the peritoneum in the stiches, so far as I have been able to observe, materially increase the risk of a general inflammation. Exemption from this, depends much more upon the habit of the animal, than the niceties of the wound.

And here I may be permitted to remark, in the way of digression, that from various observations* upon brutes, as well as upon my fellow-creatures, I cannot forbear imagining that the

* Operations for hernia and on the abdominal viscera of rabbits and dogs. The rabbit I suspect is very liable to spontaneous inflammation of the bowels. I have known in women the malignant ulcer of the womb penetrate into the peritoneal cavity, between the *rectum* and the *uterus*, without exciting a general inflammation of the belly.

risk of extensive inflammation, from local injury of the *peritoneum*, has been exaggerated, perhaps greatly. The high importance of this principle in surgery, is too obvious to require a comment; already a sufficient number of observations has been accumulated, to induce us to examine it with attention; and I may add, that it is one of those grand practical points, which ought not to be decided by a few casual facts, much less by authorities, however venerable; but, like every other principle of a solid philosophy, by various, deliberate, and unbiassed experiment and observation.

If, in performing this operation, as in the first set of experiments, the womb is divided, the incision should be made transversely near its mouth, in order that we may leave the fallopian piece as large as possible, for the reception of the *ova*, in case the genitals should have power to form them. It ought, too, to be carried from four to six lines into the mesometry, in order that the pieces thus liberated, and moving out of apposition with each other, may not reunite so as to form anew a continuous canal. If, on the contrary, as in the second scheme of experiments, the vagina is divided, a ligature should be applied to the orifice of that piece of it which remains annexed to the womb, and fastened to the margin of the external wound. This precaution ensures the escape of the thread,* and at the same time prevents the pieces of the vagina from falling into apposition, and renewing the continuity of the canal.

When the genitals are mature, the rabbit very frequently dies from this operation; which, in consequence of the large size of the vagina, is more violent than the former. It is better, therefore, on this account, as well as for reasons already assigned, to operate before puberty. Previously to this change

* In operating upon the viscera of small animals, I have occasionally used a very slender ligature, have cut it short, and left it. In two rabbits, which had apparently recovered after the vagina had been tied in this manner, a general inflammation of the belly came on about six months afterwards, in the winter, when the health of the animals was impaired by the severity of the season. On inspection after death, it was found, that the ligature still adhered to the vagina, and it seemed to form the centre from which the inflammation had spread.

the parts are comparatively small, and the interruption of the vagina does not, as we might have been led from previous reasonings to expect, prevent the subsequent developement of the sexual organs. But to return from these details.

Although it appears probable, from the preceding experiments, that the complete process of generation requires the access of the semen to the rudiments, it seems equally certain, from a variety of appearances which I noticed in the course of my experiments, that to a certain extent, though imperfectly, it may be accomplished without it. These appearances I shall now proceed to state.

In both the uterine and vaginal experiment, the womb, though it contained no fetuses, in many cases enlarged, as in extra-uterine pregnancy. Its structure too became developed; it received more copious supplies of blood; in short, it frequently seemed as well prepared as its fellow, for receiving and cherishing the rudiments.*

The ovaries, too, I may further add, although there was no genuine impregnation of them, were very obviously excited. The vesicle in different parts of them germinated; its fluids increased; the delicate covering opened; the little cavity discharged its contents, and corpora lutea formed in all their perfection. As this appearance of the *corpus luteum*, notwithstanding the interception of the semen, is of considerable importance, and may help to clear away an objection to which the experiments lie open, it becomes necessary to examine it with attention.

The corpus luteum in the rabbit, as long as it remains, is, I think, *always* marked by pretty strong characteristics, though its appearance differs considerably with its age. A mammillary projection of the ovary, an augmented vascularity, a minute cavity, which, when the luteum is cut through, recals to mind the appearance of a printed asterisk (*), constitute the leading characteristics; and by these, I may add, it is so decisively marked, that, although the parts are on a small

* It deserves notice, that, in the uterine experiments, it was generally the fallopian portion of the womb to which the semen was not applied, and not the vaginal, to which it was applied, which appeared to undergo these changes in the highest degree.

scale, an experienced eye may detect it at a glance. Colour is of little use in distinguishing these bodies in the rabbit. The younger the *luteum* is, the more prominently the characteristics appear.*

Now, these lutea, thus characterized, were distinctly produced both in the uterine and vaginal experiments. In the uterine experiments I had an opportunity of contrasting those of the fruitful and sterile ovary with each other, and yet, after the most deliberate examination, I could not discriminate the slightest difference between them. It deserves notice, also, that in some instances they were more numerous upon the prolific, and in others upon the barren side of the genitals.

In these experiments, it may be further remarked, the fallopian tubes, as well as the ovaries and wombs, seemed to be excited by coition. I observed repeatedly, in those experiments in which the vagina was interrupted, that the abdomen of the doe enlarged in a few days after the sexual commerce; and that enlargement, never noticed before, and gradually decreasing† in a few weeks afterwards, if the male was excluded,

* In giving the name of *corpus luteum* to the appearance here described, I merely adopt the nomenclature of preceding physiologists; and in stating my belief that this appearance is the result of impregnation, or, at most, of the sexual excitement when exalted to its highest pitch, I am only advancing an opinion, which is, I conceive, *as far as respects the rabbit*, confirmed by observation. I have frequently examined the ovaries of the doe, in the virgin condition, and during heat; and in one or two cases, after the animal had been under the influence of long continued and lively desire. In the two last instances I have never found the appearances described, though I dare not, from a negative observation of this kind, deny, that, under these circumstances, their formation is possible. In the first case, on the contrary, I have invariably discovered them, and older or younger in their appearance, according as they were examined sooner or later after impregnation. There can, therefore, I apprehend, be little doubt, that these appearances occurring *in the rabbit*, are the result of conception. This fact is sufficient for my reasoning. It may, indeed, seem irreconcileable with the opinion which a veteran physiologist has formed, respecting the nature of the human corpus luteum; see Philosophical Transactions; but so long as it appears to be confirmed by observations, conformably to sound philosophy, it cannot be denied. I am far, however, from wishing rashly to impugn the opinion of Sir Everard Home. Truths once proved must be admitted, and their apparent inconsistency demonstrates our ignorance, not their incompatibility.

† It did not however subside completely.

might by repeated coitions be carried to a very great degree. There is now in my possession, a doe with an interrupted vagina, which has admitted the male from twenty to thirty times. In this animal, in consequence of these repeated connections, the abdomen has gradually acquired so large a size, that it considerably exceeds the bulk of mature gestation, and reminds one of the tumour of an ascitic which requires the trocar. These enlargements, I have ascertained from repeated dissections, result from the accumulation of a humour in the wombs. This humour, various in its consistency and colour, is, however, generally fluid and pale, and turbid; and always, so far as my experiments have extended, forms albuminous concretions at a temperature below boiling heat. Even in the uterine experiments, for the preceding remarks refer to the vaginal only, the same essential appearances were observed; the wombs, in consequence of impregnation, became filled, on the sound side, with fetuses, and on the barren with the humour described.

These facts are very significant. The formation of the lutea, the developement of the wombs, and above all, the repeated accumulations of fluid there, in consequence of coition, all seem to indicate the descent of the rudimental material; and reflecting upon them, I cannot forbear imagining that the tubes were excited, that they really transferred the rudiments to the womb, and that these rudiments engendered the watery accumulations there, in the abortive attempts of generation. This notion receives some little countenance from the generation of oviparous animals; for in many of the different species referred by naturalists to this class, the rudiments may be discharged independently of preceding impregnation. The common fowl is an example of this; the frog, the toad, and a numerous tribe of fishes. This opinion, however, is merely conjectural, and I must acknowledge candidly that it is the less entitled to confidence, as it rests on a sort of accidental observation, made subordinately, perhaps with some degree of remissness, at a time when others of greater importance in the inquiry occupied a principal share of my attention. This remark I take the liberty of introducing here, as I conceive it to be the duty of every experimental inquirer *himself* to dis-

tinguish between his conjectures and demonstrations; and thus, by the exercise of a philosophical frankness, to prevent error from insinuating itself from its association with truth.

On the whole, then, it seems probable, judging from the appearances related, that generation may be carried forward to a certain extent, although the access of the semen to the rudiments is intercepted. Under these circumstances, the young animal cannot be formed, it is true; but corpora lutea may be generated; the wombs may be developed; and the rudiments, if we may judge from the facts already stated, may even be transferred to the uterine cavity by the play of the fallopian tubes.

It should be remarked, however, in dismissing this part of our subject, that these imperfect attempts at generation do not always equally occur. Corpora lutea, I believe, will be found to form invariably after sexual intercourse, if the genitals are excited at all; but in some anomalous instances, there is no consequent developement of the wombs, and in others, no accumulation of the uterine fluid. The first of these failures has occurred to me once in twelve experiments, and the last of them five times.* But these *negative* irregularities merely prove, that, under circumstances, the genitals may be more extensively excited at one time than another. They by no means invalidate the principle which it has been my endeavour to establish on positive facts, that the ovaries, tubes, and *uterus*, are capable of an imperfect excitement, even when the semen and the rudiments are kept apart from each other.

Against the experiments and reasonings advanced in the preceding pages, various objections may be urged, to which it may now be proper to advert.

And first, it may be objected that sterility is sometimes an accidental occurrence. We frequently observe it in human generation. In the experiments under consideration it would perhaps have occurred, although the interception of the semen, to which it is ascribed, had not taken place. To these objections, however, I would reply; that in the rabbit the acciden-

* In one or two instances the orifice formed by dividing the uterus remained open in the fallopian piece. This accounts for some of the failures of uterine accumulation.

tal failure of impregnation is rare, and does not occur in one doe out of twenty, if the animal is in health; that the appearance of the genitals, and the behaviour of the female when the male was admitted, both of them indicated inclination and aptitude for generation; that these experiments were not solitary, but frequently repeated; and that sterility was not an accidental occurrence, in a single instance only, but an invariable result of them all. Nor must it be forgotten that the formation of the *lutea*, and the evolution of the *uterus*, are themselves sufficient proofs that the genitals were not accidentally inactive; nor that in the uterine experiment, in which the semen was intercepted on one side only, there were undeniable proofs of the generative excitement in the formation of the young animals on the other.

But there is another objection to which the experiments lie open, which, on a cursory consideration, at least, may appear to bear with considerable weight. In these operations either the wombs or the vagina were cut asunder. It may be asserted therefore, that sterility ensued, not so much in consequence of the interception of the semen, as from the debility induced in the genitals by operative violence; the germs afterwards perishing because the soil was become unfriendly.

To this plausible objection, however, it might be sufficient to reply, that from the form of the parts the injury of the operation is merely local; that when the vagina is cut through, before puberty, the genitals suffer so little from it that they are afterwards brought to maturity in the same manner as if no operation had been performed; and that in both sets of experiments, whether uterine or vaginal, the wombs frequently become enlarged and developed, and like a fruitful and well dressed soil, to resume the figure already adopted, are brought into high condition for raising the rudiments to perfection. To obviate this objection, however, in a still more satisfactory manner, the following experiments were instituted.

I divided the vagina of two young does, just before their puberty; but instead of securing the uterine piece to the verge of the abdominal wound, I allowed it to remain in apposition with the other. In consequence of this method of operating

the parts reunited; the canal of the vagina was renewed; and the sexual desires appearing a few weeks after recovery, both the rabbits became impregnated. The inference is obvious.

The second set of experiments, turning on the same principle, was executed on the wombs themselves. In these, both the wombs were divided, the one in two, and the other in three places; in such a manner, however, that the incision was not carried completely across into the mesometry; so that the pieces were retained in mutual apposition, and reunited without interruption to the uterine canal.

The result of these experiments was decisive. From the very method of operating it is obvious the wombs were more roughly handled in this than in any of the preceding experiments; accordingly a larger number of the rabbits died; and yet, notwithstanding this violence, the very first doe which recovered produced no less than nine fetuses from her *first* intercourse with the male. Indeed so complete was the action of the *uterus*, that there was not one of the little masses of rudimental matter which it failed to mature; and it was found, on a careful comparison of the wombs with the ovaries, that the number of fetuses and corpora lutea was the same. To these remarks I may add, that the human womb, although it has been cut or torn, or partially destroyed by ulceration, still retains the power of maturing the rudiments. Healthy children have been born, not only after recoveries from uterine rupture and the Cesarian operation, but even at the time when the neck of the womb had been ulcerated. A case of this kind has lately fallen under my own knowledge; and others are recorded by obstetric writers.

There yet remains a third objection, which, it is conceived, may be completely obviated, though at first view it wears a very formidable aspect. The vagina of the rabbit is very long and very large; its course is not direct; the organ of the male can neither fill it nor penetrate to the orifices of the wombs; how then can the semen be injected into the uterine cavity, even granting that it might meet the rudiments there?

This objection, felt in all its force by those who have examined the genitals merely in the preparation glass, falls at once when they are viewed in the rabbit while it is living; or,

to avoid unnecessary severity, immediately after the dealer has killed it.

Both the vagina and the wombs perform a peristaltic action, the wombs somewhat obscurely, the vagina in a more lively manner, even than the intestines of the animals themselves. This canal indeed, during the heat, is never at rest; it shortens, it lengthens, it changes continually in its circular dimensions; and, when irritated especially, will sometimes contract to one third of its quiescent diameter. Now this peristaltic action, resembling the intestinal, is itself sufficient to explain the transmission of the semen.* In addition to this action, however, the vagina performs another, easily comprehended on inspection; although, as frequently happens, the verbal description of it may perhaps appear a little obscure. The action to which I here allude, consists in the falling down, as it were, of that part of the vagina which lies in the vicinity of the wombs; so that it every now and then lays itself as flatly over their orifices as we should apply the hand over the mouth, in our endeavours to stop it. So close is this application, that I have sometimes fancied I could perceive externally something resembling a little dimple, occasioned by the sinking of the surface of the vagina into the orifice of the womb. How well adapted the whole of this curious movement is for the introduction of the semen at the opening, it is needless to explain. The mere performance of it furnishes no contemptible argument in proof of that approach of the semen to the rudiments for which I have been contending.

Before I close these observations, already perhaps too diffuse, I cannot forbear adverting to some other points of the genital physiology, which they may contribute to illustrate.

It has been asserted by some naturalists, that the corpus luteum is an evidence of genuine impregnation. It seems *certain*, however, from the facts related, that this evidence cannot be relied on; for the luteum, in these experiments, was

* There is some little reason for surmising that even the human vagina can perform a sort of peristaltic movement. Two facts have been related to me which lead to this opinion, but they are of a character too delicate for public exposure.

generated under circumstances in which, as the event proved, impregnation was impossible. Indeed there seems to be little reason for doubting, that the corpus luteum may be produced, even independently of the sexual intercourse, by the mere excitement of desire in a very high degree. Mr. Saumarez has recounted experiments, in his "New System of Physiology," in which the *luteum* appears to have been generated in this very manner. I have now in my possession a preparation, for which I stand indebted to Dr. Cholmeley and Mr. Callaway, consisting of the ovaries of a young girl, that died of chorea, under seventeen years of age, with the hymen, which nearly closed the entrance of the vagina, unbroken. In these ovaries, the corpora lutea are no fewer than four. Two of them, it must be acknowledged, are a little obscure; though an experienced eye, I conceive, would readily detect them. The remaining two are very distinct, and differ from the corpus luteum of genuine impregnation, merely from their more diminutive size, and the less extensive vascularity of the contiguous parts of the ovary. In every other respect, in colour and form, and the cavity which they contain, their appearance is perfectly natural; indeed so much so, that I occasionally circulate them in the class-room, as accurate specimens of the luteum upon the small scale.

On this point I have been the more explicit, both as the principle is of some importance in forensic medicine, and as it removes at once an objection to which these experiments lie exposed, and which is taken from those of Dr. Haighton. In these experiments, very ingenious, and extremely beautiful, my valued relative has shown, with his usual accuracy, that the corpus luteum may form though the fallopian tube has been obliterated in some part of its course, and the access of the semen to the rudiments therefore has been intercepted. When, however, he infers from this, in opposition to the principle asserted in this memoir, that the ovary has been impregnated, notwithstanding the interception of the semen, he certainly falls into one of those errors, from which the most wary physiologist is never absolutely exempt, for the corpus luteum is not a certain evidence of impregnation.

The appearances related, I may further remark, afford,

when combined with others, a plausible proof that the semen sometimes penetrates as far as the ovaries; a point which has been much controverted.

In the varieties of human generation, we sometimes meet with extra-uterine pregnancies, in which the ovum not only lodges in the tubes, or the peritoneal cavity, but in the ovary itself. Indeed, this form of the disease seems on the whole the most common. Now, if it be true, as I have endeavoured to prove, that the young animal cannot be formed unless the semen have access to the rudiments, it is evident, that in these pregnancies, in which the fetus is generated among the graafian vesicles, the semen must have made its way up to the ovaries themselves. It must not, however, be too hastily inferred from this, that the semen always penetrates into these remote recesses of the genitals. Facts have been related, which give a shade of probability to the conjecture, that without the contact of the semen the rudiments may sometimes descend into the uterus; and certainly, although the opinion is not without its difficulties, it is not impossible that they may meet each other there.*

There is yet a third point in the physiology of generation, which the preceding experiments may contribute to elucidate. It has been contended by some naturalists, and not without show of reason, that the semen in generation is transferred to the blood-vessels; and as the purgative or emetic, when injected into the veins, exert their peculiar influences on the stomach or the bowels, so also, in their opinion, this active fluid, transmitted by the absorbents, makes its first impression on the vascular surface, and its second, by a similar sympathy, on the genitals themselves. What effects might be produced by injecting the semen directly into the veins when the genitals are in a state of excitement, I shall not venture to determine; as yet I am in possession of no decisive experiments upon the point, and it would be a mere waste of mind to speculate without them. It seems evident, however, from the facts related, that after transmission through the absorbents and their glands,

* Is the transfer of the semen beyond the womb the cause of extra-uterine pregnancy?

the semen retains no such generative influence. It will be readily conceded, that when a rabbit admits a large male, in vigorous health, and in the flower of its age, as many as twenty or thirty times, a large quantity of the genital fluid must be imbibed by the absorbents of the vagina, yet neither in the uterine nor the vaginal experiment, in which these repeated coitions sometimes took place, was impregnation by absorption accomplished. The simple exclusion of the semen from the rudiments always prevented the formation of the young animal; in the vaginal experiments it was not produced at all; in the uterine it was formed on that side only where the womb remained pervious.

On a review of the whole inquiry, it will, I conceive, appear not improbable that, for the completion of generation, the semen must have access to the rudiments; and yet that notwithstanding the necessity of these approaches, for its completion, the process to a certain extent may be accomplished without them. These are the two leading propositions which it has been my endeavour to establish; at the same time I have subordinately attempted further to show, that the corpus luteum is not a proof of genuine impregnation; that the semen, at least occasionally, penetrates as far as the ovaries; and that however copiously this fluid may be absorbed into the vessels, it is incapable of giving rise, by any impression there, to the complete circle of the generative actions.

Whether these principles of brute generation may be transferred to our own, I will not venture to determine. Analogical arguments, generally the best that physiology furnishes, are, it must be admitted, never absolutely demonstrative; but as the generation of the rabbit, in its other principles, resembles that of the human female, there seems to me but little reason for supposing that there is an essential difference here.

On the Doses of the Ancient Physicians. By E. Milligan, M. D. Lecturer on the Institutions of Medicine.

[From the Edinburgh Medical and Surgical Journal, for April, 182 0]

Happening, during last summer, to look into some ancient writers on medicine, my attention was particularly attracted by a feature of their practice of which I can no where meet with a satisfactory explanation. As the subject seems curious, it may, p rhaps, be worth while to notice briefly in this Journal the results of a hasty inquiry into its origin and reality. To those possessed of more ample means of information they may appear jejune and puerile: and if any such condescend to correct their errors, by the same channel in which they are now humbly offered to their notice, I shall candidly acquiesce in this imputation.

The difficulty here alluded to occurs in the *doses* of medicines exhibited by the ancient physicians. These have long been observed to be very large;* and it was on remarking the impossibility, according to our ideas, of exhibiting them with safety to human life, or even of escaping destruction under their influence, that my observation was first drawn to the subject of ancient posology.

Dioscorides, who lived under Nero, and who, as he informs us, had attended to the subject of the materia medica from his earliest years, may be taken as indisputable authority on this point; especially, as we are informed by Suidas, that he practised as well as studied medicine;† and his work stood in such high estimation among the ancients as to be copied literally in many parts by Pliny, though almost a contemporary. Indeed, whoever peruses his own excellent work on the materia medica with attention, will seek little farther for proof of his accuracy and diligence; and he and Pliny must always be cited as examples of the triumph of genius and industry over the harassing toils and interruptions of a military life.‡ It was not the intention of Dioscorides to deliver formally the dose of each medicine along with its natural history and

* Arbuthnot on Ancient Weights and Measures, p. 286.

† By himself, also indirectly, De ver. albo. lib. iv. ‡ Dios. Praef.

medical application, and, therefore, it is only occasionally that we find him particularly describe the quantities and forms in which they must be exhibited. He, indeed, explains in one place why the doses were not comprehended in his work; and as the passage leads to a curious fact in the history of physic, we shall deliver it in his own words. Whilst treating of the white hellebore, a most important article of ancient materia medica, he adds, "Mensura dispensatioque ejus ab HIS tradita est, QUIBUS præcipuum fuit studium de eo (περὶ τῆς δόσεως αὐτῆς,) docere. Nosque Siculo ab Ethna Philonidæ in ea re subscribimus, longum enim fuerit materiam medicam ex proposito tractantes, faciendae medicinae rationem docere."* Hence it appears that the ancients carefully separated the posological part of medicine from the materia medica; and with good reason; as, besides being too bulky an addition to the multifarious information of that science, it would have put the more ostensible part of the art into the hands of the vulgar, as the uninitiated of those times dabbled quite as much in physic as they do at present. We see likewise that they were in possession of distinct treatises on the subject of posology, and that the person here mentioned, Philonides the Sicilian, was an eminent author in that department. This passage likewise explains to us why Hippocrates, Celsus, Dioscorides, Pliny, and all the earlier writers on medicine, so rarely mention the dose even of the most powerful medicines; and, without some such key to their motives, Dr. Arbuthnot† might well wonder at Hippocrates so carefully dosing his slops and asses' milk whilst the most acrid remedies are apparently left to the discretion of the practitioner or his patient. I shall, however, in what follows, endeavour to throw some additional light on this peculiarity of the ancient physicians, though, from the above, it is evident that a reference to books of posology was always implied in the passages where the quantity and circumstances in which a medicine should be given are not specified by these authors.

Wishing to discover, if possible, whether some general

* Dios. Lib. IV. de veratro albo.

† Ancient Weights and Measures, 288.

multiple of our present doses might not be detected in the doses of the ancient physicians, I selected from the work of Dioscorides, all the medicines of whose composition and identity no doubt can be entertained, noting particularly such as must, when given in excessive quantities, produce the most violent effects. The history of each of the *latter*, as given by Dioscorides, was then carefully inspected, and the doses, with their proposed object written down. A medium of them all, if not already given by the author himself, was afterwards computed; and a corresponding medium dose, at the same time, extracted from the posological table given by Dr. Duncan, junior, in his excellent Dispensatory. The results were set down in the following table, wherein the drachm is taken at sixty-two grains, as ascertained by the famous antiquarian Greaves,* and admitted by Dr. Arbuthnot† as nearly correct. To enable the general reader to compare these measures of the text with the modern troy weight, another table is subjoined, containing most of the medical weights and measures anciently in use.

Comparative table of Ancient and Modern Doses.

ALOES. In haemoptoe, cochl. ij. In icterus, ob. iij. ad ʒi.; for free purging ʒiij.

In grains 310 31 62 186 medium 62, mod. med. 15, but $\frac{62}{15} = 4.133$

SULPHATE OF COPPER, for worms ʒi.

In grains 62 0 0 0 ——— 20 ——— 5 — $\frac{20}{5} = 4.000$

ELATERIUM, common dose, ob. j. smallest dose, ob. ss. to infants, chalc. ij.

In grains $10\frac{1}{3}$ $5\frac{1}{6}$ 2 0 ——— $10\frac{1}{3}$ ——— 2 — $\frac{10\frac{1}{3}}{2} = 5.166$

OXIDE OF COPPER, as an expectorant, ob. iij.

In grains 31 0 0 0 ——— 20 ———say 5 — $\frac{20}{5} = 4.000$

SCAMMONY, for a purge, ʒi. aut ob. iiij. for a laxative ob. ij.

In grains 62 $41\frac{1}{3}$ $20\frac{2}{3}$ 0 ——— 30 ——— 8 — $\frac{30}{8} = 3.750$

* Dissert. on Roman Denarius.

† Ancient Weights and Measures. He makes it $62\frac{2}{3}$

SQUILL, to purge off bile, ob iij.

In grains 31 0 0 0 medium 10, mod. med. 2 but $\frac{10}{2}$ = 5.000

BLACK HELLEBORE, to purge off bile or pituita, ℥i, or ob. iij.

In grains 62 31 0 0 —— 42 —— 15 — $\frac{42}{15}$ = 2.800

7)28.849

Mean quotient, 4.121

Table of Ancient Medical Weights and Measures, reduced to their equivalents in Modern Troy Weight.

1 sextons = 1 obolus = 10 chalci* = 10⅓ grains.

1 drachm = 1 denarius = 6 sextantes = 6 oboli = 60 chalci = 62 grains.

1 uncia = 3 cochlearia = 7 drachms† = 7 denarii = 42 sextantes = 42 oboli = 420 chalci = 434 grains.

1 cyathus = 4 cochlearia = 10 drachms = 10 denarii = 60 sextantes = 60 oboli = 600 chalci = 620 grains.

1 cochleare = 2½ drachms = 2½ denarii = 15 sextantes = 15 oboli = 150 chalci = 155 grains.

1 acetabulum = 15 drachms = 15 denarii = 90 sextantes = 90 oboli = 900 chalci = 930 grains.

1 hemina = 60 drachms = 60 denarii = 360 sextantes = 360 oboli = 3600 chalci = 3720 grains.

1 sextarius = 2 heminae = 120 drachms = 120 denarii = 720 sextantes = 720 oboli = 7200 chalci = 7440 grains.

1 congius = 6 sextarii = 12 heminae = 720 drachms = 720 denarii = 4320 sextantes = 43200 oboli = 43200 chalci = 44640 grains.

The candid reader will easily perceive, that the numbers given in the former table as modern medium doses are not meant for *moderate* doses of the medicines there mentioned; they are merely the arithmetical mean of the extremes given

* My reasons for dividing the obolus into ten chalci, and not into six, as Arbuthnot and others have done, will be given at length in my edition of Celsus; meantime the reader may consult Pliny XXI. 34.

† Not eight drachms, as the common ounce. The excellent Arbuthnot, (284) was misled by not comparing the above passage of Pliny with Celsus. V. 18, 1.

in Dr. Duncan's table; for this being the mode in which I have generally assumed the medium of the ancients, I thought it fair to compare it with the modern medium obtained in the same manner; besides the advantage of having the authority of a physician and author so justly celebrated in this department. It produces no other difference in the general result than that of making the disproportion between the ancient and modern doses too small. As Dioscorides appeared the most authentic and judicious evidence, his doses merited the first place; his numbers, besides, are often written in compound Greek words, so as to preclude all possibility of mistake by the transcriber. But, on proceeding to examine the other authors who have given the doses of violent medicines, we shall find the coincidence appear so perfect as to remove any shadow of doubt that might still remain respecting the single testimony of Dioscorides.

The most perspicuous and precise author of all antiquity is Celsus; and being engaged at the time of the enquiry in preparing an edition of that author, I set about examining the few doses which he has delivered.

1. The famous antidote of Mithridates consisted of thirty-five different ingredients, mostly aromatics and bitters combined with opium, so that 10743 gr. contained 248$\frac{1}{3}$ of opium, or $\frac{1}{43}$. Celsus (179) gave this as an anodyne to the size of an Egyptian bean, whose dimensions, indeed, are not well known, but which, as Dioscorides informs us, was larger than a common bean. Rhodius, ad Comp. Larg. 95, makes it of the same weight as a victoriatus, or two scruples; but he makes the common bean a drachm, *Ad Scrib.* 13, and, therefore, we must consider the Egyptian bean as heavier than a drachm. But as an anodyne it was given to the bulk of an almond, or four times the size of the former, in which case it would contain about six grains; for $\frac{62}{43} = 1\frac{1}{2}$, and $1\frac{1}{2} \times 4 = 6$ grains nearly, which, instead of being an antidote for poison, might, in modern times, itself prove fatal; at any rate, the party who previously swallowed such a dose must have made a poor figure at those splendid but treacherous entertainments of the ancient

great, where death so often lurked in the cup which the Dii Geniales had in vain consecrated to friendship and festivity.*

2. The cough mixture of Athenio, 248 grains to 62 of opium, dose, an Italian bean. Now, $\frac{248}{62} = \frac{4}{1}$. Call the bean even ℈ij. or 40 gr. and $\frac{40}{4} = 10$ grains of opium in the dose.

3. Another cough mixture had opium $\frac{248}{62}$, or one in four parts as before, and the same dose, or rather its double, being two catapotia of the size of a common bean; therefore, = 20 grains.

4. A composition for Ischuria had opium $\frac{4298}{434} = 1$ part in 9.9 or 10; but $\frac{62}{10} = 6\frac{1}{5}$ grains of opium for a dose; for this is what was contained in the Egyptian bean.

Throw these together as above, and we have

Table of Doses from Celsus.

Mithridate, a dose of	6 gr.	$\frac{6}{4} = 1\frac{1}{2}$
Athenio's cough-mixture,	10	$\frac{10}{4} = 2\frac{1}{2}$
Another do.	10	$\frac{10}{4} = 2\frac{1}{2}$
A mixture for ischuria,	$6\frac{1}{5}$	$\frac{6\frac{1}{5}}{4} = 1\frac{3}{4}$

We see by the above table, that when the enormous doses of Celsus are divided by 4, which is the quotient of the mean ancient doses by the mean modern,† that they then assume a rational magnitude, and, indeed, such as would be given at the present time on his indications. Yet I am not inclined to insist much on the force of this coincidence, as the evidence from other authors is much more direct.

Hippocrates, like the rest, rarely mentions doses; but when he does, it is exactly in the same excessive ratio. He gives an obolus, or $10\frac{1}{2}$ grains of elaterium, in a female case.‡ His other very few doses specified are not of substances that afford any decisive information to our inquiry.

* "When thou sittest to eat with a ruler, consider diligently what is before thee; and put a knife to thy throat if thou be a man given to appetite. Be not desirous of his dainties, for they are deceitful meat."—Prov. XXIII. 1, 2, 3.

† P. 351, supra.

‡ De Superfœtatione.

Scribonius Largus gives a catapotium for a cough with expectoration, in which the opium makes $\frac{1}{2}\frac{3}{7}\frac{7}{1}\frac{3}{8}$, or $\frac{1}{2}$ of the whole composition, yet he gives three or four pills of the size of a vetch pea in the course of the night. Say the pill weighed only 2 gr., yet 2 × 3 or 4, will give 6 to 8 grains of opium for one night's dose.

The same author gives another catapotium for an old cough, which has $\frac{6}{1}\frac{2}{3}\frac{}{7}$ of opium, or $\frac{1}{2}$, and this is likewise given three or four times a night to the size of a bean, amounting to nearly a scruple of opium!

His cough pastilli, or lozenges, weigh 31 grains, of which the $\frac{1}{7\frac{1}{2}}$ is opium, or each lozenge contains four grains. For Ischuria he gives a drachm, or 62 grains of a medicine which has $\frac{62}{11}$ or $5\frac{1}{2}$ grains of opium in it.

Marcellus lived under the emperors Gratian and Theodosius, and is said to have been an empiric. He gives 31 grains of aloes for a laxative, and this in many formulæ.

Of a purging medicine, $\frac{2}{3}$ of which consisted of scammony and black hellebore, he gave the size of a hazle-nut, probably a drachm.

He gave troches weighing ʒi. in nephritic pains, and the $\frac{1}{6}$ of these was opium; hence $\frac{62}{6} = 10\frac{1}{3}$ grains of opium in each dose.

Rufus Ephesius orders pulp of colocynth for a purge ʒi. or 62 grains. Aloes ʒij. or 124 grains of the juice of tithymallus (very acrid) ʒi. = 62 grains. Paulus Aegineta gave for purging medicines the following doses: of Aloes ʒi. = 62 grains; of black hellebore, ʒi. = 62 grains; of scammony, 4 oboli, or 42 grains; of colocynth, ʒi. = 62 grains; of elaterium, 3 oboli, or 31 grains; of the oxide of copper, 31 grains.

The same author gives compound purgatives, with doses agreeable to the simple ones just given. Thus the famous "Purgatoria ex Hermodactylo Podagrica," or arthritic purgative of colchicum, now ascertained* to be identical with the "Eau Medicinale d'Husson," is given by him as follows:

* Vide Medico-Chirurgical Transactions.

"PURGATORIA EX HERMODACTYLO PODAGRICA."

"Hermodactyli quadrantem (= ℥iij.

Anisi, Cumini Æthiopici, ameos, thymi corymborum, piperis albi, zingiberis, singulorum ℈iij., epithymi ℥ss. Dosis ℈iv.—aliqui sex dant. Dantur manè cum condito, aut aqua mulsa, aut mero fervefacto."

On calculation, it easily appears that one-third of the composition is colchicum root; and the less daring practitioners, therefore, who ordered ℈iv. of this medicine were giving $\frac{81}{3}$, or 27 grains of colchicum root to their patient.

Aretæus seems to give similar doses, but, like the rest, mentions very few. To bring the whole of these observations into one view, we shall throw them into a tabular form.

Tabular view of Ancient Doses continued.

I. HIPPOCRATES.

Elaterium, in dose of	10½ gr.	but $\frac{10\frac{1}{2}}{4} = 2\frac{1}{2}$ gr.

II. SCRIBONIUS LARGUS.

Cough pill, having of opium	6 to 8 gr.	$\frac{6\text{-}8}{4\text{-}4} = 1\frac{1}{2}$, 2 gr.
Old cough-pill, do.	20	$\frac{20}{4} = 5$
Cough-lozenge, do.	5½	$\frac{5\frac{1}{2}}{4} = 1\frac{1}{2}$
Medicine for ischuria, do.	5½	$\frac{5\frac{1}{2}}{4} = 1\frac{1}{2}$

III. MARCELLUS.

Aloes for a laxative, in many formulæ,	31 gr.	$\frac{31}{4} = 8$ gr.
Purge of Scammony & Hellebore,	31	$\frac{31}{4} = 8$
Nephritic Troches, having opium	10½	$\frac{10\frac{1}{2}}{4} = 2\frac{1}{2}$

IV. RUFUS EPHESIUS.

Purge of Colocynth, pulp.	62 gr.	$\frac{62}{4} = 15$ gr.
Aloes,	124	$\frac{124}{4} = 31$
Juice of Spurge	62	$\frac{62}{4} = 15$

V. Paulus Aegineta.

Purge of Aloes,	62 gr.	$\frac{62}{4} = 15$ gr.
Black Hellebore,	62	$\frac{62}{4} = 15$
Scammony,	42	$\frac{42}{4} = 10\frac{1}{2}$
Colocynth,	62	$\frac{62}{4} = 15$
Elaterium,	31	$\frac{31}{4} = 8$
Oxide of Copper,	31	$\frac{31}{4} = 8$
Colchicum root,	27	$\frac{27}{4} = 6\frac{3}{4}$*

From all these facts taken together, it appears that the ancients, from Hippocrates downwards, were in the habit of giving doses at least four times stronger than ours; and that this ratio obtains with remarkable uniformity through all their works, embracing a period of time little short of a thousand years. The coincidences are so numerous that no one can ascribe them to chance; indeed, we can easily trace the dose of Dioscorides and the older writers copied into the pages of the latest, as of Paulus and Marcellus. Let it not be imagined that these extraordinary doses are culled from amongst a great many where the quantity was moderate. I have no where done so, but have always noticed their ordinary doses when the ancients themselves mention them as such. Where, on comparison with the ancient authors, the learned reader may form a different opinion, I would beg him to consider that we, at this time, can only judge of their mode of prescribing by what I call the *impossible* doses, just as we arrive at truth in other sciences by the "*reductio ad absurdum.*" It is on this mode of reasoning, and the discovery of a secret quadruple subdivision of doses, that the interest of the present inquiry, if it has any, must rest; for the labour of turning over ancient books, and

* I have not taken the trouble to calculate Dr. Duncan's arithmetical mean corresponding to these doses, but the reader can easily do this for himself.

comparing examples, is a drudgery too trite and mechanical to deserve much attention from the readers of this studious, intellectual age.* He must remember that no judicious physician of the present day prescribes an active medicine to its utmost dose in compositions wherein it is only of collateral benefit; and our posterity would judge very much amiss of our ordinary doses of opium, if they were to deduce them from the dose of the London paregoric, or the electuarium catechu compositum of the Edinburgh pharmacopœia. It follows, that occasional moderate doses, did they even occur more frequently in ancient authors, could not decide the matter at issue.

On what principle, it may be asked, are these extravagant, nay, murderous doses of the most acrid and poisonous matters to be explained? Does there, after all, exist some mistake in the numbers from long continued and careless transcription? or do we entirely mistake the true weight and relation of the Roman denarius† to our drachm troy?—Did the ancient physicians really exhibit these doses to patients who, from early habits of activity and exercise then almost universal, had bodies able to resist their influence? Or, did they in order to conceal the art from the profane vulgar, purposely falsify, and for convenience do this in a certain quadruple ratio?—employing weights graduated indeed like those in common use, but secretly understood to be subject to this fourfold division? Each of these conjectures may seem to merit attention, and I shall close this communication with a few remarks on their individual probability.

I. That the doses above, extracted from the ancient authors, have not been altered by the transcribers, is clearly shewn by the fact, that many of them are not in characters, or numeral abbreviations, but in written words, Greek compounds, such

* Accordingly, wherever it was convenient, I have borrowed freely from the labours of Arbuthnot, just as any one is at liberty to borrow from these.

† The Roman denarius is still extant in abundance; and its relation to the other weights is so precisely described by the ancients, that its weight troy being known, all the others are obtained in grains by simple proportion. See the table given above. Pliny, xxxiv. 5, seems to insinuate that their alloy was one-eighth in his time, though modern chemists maintain that the ancient coins are nearly pure.

as the word τριόβολος, for example, which could not, by any conceivable chance, have been substituted for its fourth part or eight grains; yet this is the term by which Dioscorides denotes the dose of squill requisite to purge off the bile. Indeed, had any error crept into the text of the older authors, it would never have been copied into the succeeding writers, who were generally practitioners, and could not mistake the dose of medicines so familiar, and whose mode of preparation is so precisely described.

II. There may still seem grounds for believing that the moderns have not a correct estimate of the weight denarius, and that there is, therefore, no accurate method of ascertaining the absolute relation of the ancient weights to those of our times. But the ancient denarius or drachm, for they are equivalent terms, was a common Roman money, about the weight of three sixpences of our present* coinage, and is still to be met with abundantly amongst the dealers in old coins; and numbers of them exist in various public collections, in a state of high preservation. Mr. Greaves, the famous antiquarian, weighed many hundreds of them in different countries, and found the medium weight of the denarii consulares to be very steadily 62 grains, whilst the attic tetradrachm, corresponding to our crown piece, and which ought to weigh four times 62, or 248 grains, at an average weighs four times 67, or 268 grains, making the drachm 67 grains. The latter, however, is evidently a provincial variation, of which we have abundance of examples in our own country and times;† indeed, the measure of the congius of Vespasian, a standard vessel which is still in existence, agrees so exactly with the former weight of the denarius, 62 grains, that there cannot exist a rational doubt of its exactness. A judicious and learned friend, who has spent a considerable time in Italy, and whose connections and pursuits naturally gave an interest to the inquiry, found the

* 1816.

† The cause of this difference seems to have been the alloy, which is commonly one-twelfth, and which the ancients probably did not consider of any value; and, therefore, only estimated the weight of these coins by the pure gold or silver contained in it. This supposition is simple, and explains the difference exactly.

present drachm used by the apothecaries of Italy, to correspond exactly with the ancient Italian denarius or drachm, ascertained by Greaves, consisting of 62 grains troy. Indeed, it is natural to suppose, that, of all other places, the apothecary's shop must be that where uniformity would be longest and most exactly preserved; deviations there being attended with the most immediate and fatal consequences, whilst the usual motives for change are totally wanting, their goods being generally bought in by a different weight. The same excellent physician conjectures, and with great probability, that, were any one to make the experiment, the above weight of 62 grains troy would still be found in the apothecaries' drachm of modern Greece, unaltered, perhaps, since the days of Aristotle.

III. Did the ancient physicians then, really exhibit the above doses to their patients, who, from early habits of activity and exercise, then almost universal, may be supposed to have had bodies able to resist their influence? This explanation cannot, I fear, be admitted. It is incontestably established that the bodies of the Greeks and Romans, whose physic we are considering, were not larger than those of the present race of mankind; nay, more,—we have the constant unbiassed evidence of their own historians, orators, and poets, that their stature and bodily strength was considerably inferior to that of the Asiatics of the south, or of the Gauls to the north of their country. As it is probable that little change has taken place in the personal strength of the latter, who were true Celtae, that tribe, like the Jews, having a wonderful tendency to retain their primitive habits; so, reasoning upwards, it follows, that the ancient Greeks and Romans must have possessed less personal vigour, and therefore, must have less resisted the action of excessive doses than a modern Welshman or Scotch Highlander, whose vis medicatrix would, notwithstanding, be somewhat staggered at a 20 grain dose of opium or sulphate of copper. Much, however, may be said on the other side of the question; and the almost total absence of diseases of debility, of scorbutus, the cutaneous tribes, affections of the heart, of typhus fever, of scrofula, of rickets, of chronic rheumatism, in the medical works of the ancients, would seem to

savour something of modern degeneracy.* Their lives were much more debauched than ours, and had their constitutions been as weak, the same consequences must have been observed. Indeed, their general practice of vomiting in order to sharpen the appetite must have given such a shock to the system as few of our modern gourmands would like to undergo for the enjoyment in prospective; yet this practice had prevailed from the most early times, since Hippocrates alludes to it, but thinks it wiser to vomit but once a fortnight. The instances of debauch given by their authors far exceed the eating and drinking matches of the English newspapers,—nay, many of them were habitual. Thus Cato allowed his slaves individually, during the Saturnalia, seven pints, or four bottles of wine, per diem, which would appear in this age rather a liberal allowance. Cicero's son was called Bicongius, because he was accustomed to drink two congii off at a draught. But two congii are seven quarts or eight bottles of wine! the wine may have been weak, but who of these times could have drank off as much water with impunity? Pliny and others abound in such examples, many of them much more wonderful, the present being selected merely for the eminence of the individual and the impossibility of mistake in respect of quantity. Lastly, we find the most execrable instances of corporeal and mental depravity among the ancients enjoying uninterrupted health. The infamous tyrant and debauchee, Nero, was only three times sick in fourteen years. "Nam qui luxuriae immoderatissimae esset, ter omnino per XIV. annos languit; atque ita, ut neque *vino*, neque consuetudine *reliqua* abstineret;†"—and the like is reported of many others. Even the satirists, whose style was exaggeration, and whose proper subjects were vice and luxury, never mention other consequences than crapula or indigestion, from the disgusting gluttony of the times; very rarely even gout. Notwithstanding all this, however, we must incline to the former opinion, that the ancients were less robust than we are; it being now well ascertained,‡ that of all the two legged varieties of the creation, whether wild or tame,

* This is a difficult passage. Should it not be—would seem to savour of something different from modern degeneracy.

† Suet. de Nerone.

‡ See Lawrence's Physiology, Zoology, and History of Man.

an Englishman possesses the highest mascular power; and hence, *a fortiori*, stronger than the Celts, who were, and are, as we have just seen, stronger than the ancient Romans.

IV. The only remaining mode in which this difficulty can be solved is, by supposing that, in order to conceal the art from the profane vulgar, the physicians of antiquity intentionally falsified their doses; and, for convenience, did it always in a certain uniformly quadruple ratio; employing weights, indeed, graduated like those in common use, but secretly understood to be subject to this fourfold division. We arrive at this inference by the method of exclusion, for we have seen above, that every other conceivable mode of accounting for the excessive doses of ancient medicine is without foundation. In supposing that this was the case, we do no more injury to the morality of the ancients, than to the moderns, in allowing that the practice of prescribing in Latin words and Roman characters is for the same purpose;* a thing which few deny, and many writers openly avow. That they *concealed* their doses is manifest from their writings, and from the singular fact, that though we have seen above that they had choice of books on posology, not one of these have reached us, either of Greek or Roman composition; being probably attainable to none but practitioners.

The facts are so obvious and striking, that Dr. Arbuthnot, in the year 1727, formed nearly the same conjecture; and if concealment was practised, it is evident it was only by falsifying the weights and measures of the day in a *constant* ratio, that one medical man could become intelligible to another; or, indeed, have a motive for naming the dose at all, which otherwise must have been completely unintelligible to his readers. A secret understanding, then, of a diminution of the dose in a fixed ratio appears necessary; and as the tables given above show plainly enough that this ratio was a quadruple one, I shall not longer insist on it here; but, with renewed request, that some one better qualified will take this curious circumstance into consideration, conclude these few remarks on a subject rather beyond my opportunities of inquiry.

South College Street, Dec. 25, 1819.

* "Scilicet qui non mulierculis aut circumforaneis medicis artem impertiar."—Greg. Consp.

An Account of a Membrane in the Eye, now first described.

By Arthur Jacob, M. D., Member of the Royal College of Surgeons in Ireland, Demonstrator of Anatomy, and Lecturer on Diseases of the Eye in the University of Dublin.

[From the Philosophical Transactions, for 1819. Part II.]

Anatomists describe the retina as consisting of two portions, the medullary expansion of the nerve, and a membranous or vascular layer. The former externally, next to the choroid coat, and the latter internally, next to the vitreous humour.* All, however, except Albinus and some of his disciples agree, that the nervous layer cannot be separated so as to present the appearance of a distinct membrane, though it may be scraped off, leaving the vascular layer perfect. That the medullary expansion of the optic nerve is supported by a vascular layer, does not, I think, admit of doubt; but it does not appear that Albinus was right in supposing that the nervous layer can be separated in form of a distinct membrane, though shreds of a considerable size may be detached, especially if hardened by acid or spirit.

Exclusive of these two layers, I find that the retina is covered on its external surface by a delicate transparent membrane, united to it by cellular substance and vessels. This structure, not hitherto noticed by anatomists, I first observed in the spring of the last year, and have since so frequently demonstrated, as to leave no doubt on my mind of its existence as a distinct and perfect membrane, apparently of the same nature as that which lines serous cavities. I cannot describe it better, than by detailing the method to be adopted for examining and displaying it. Having procured a human eye, within forty-eight hours after death, a thread should be passed through the layers of the cornea, by which the eye may be secured under water, by attaching it to a piece of wax, previously

* Ruysch. Epist. Anat. Prob. xiii. Albinus, Annot. Acad. lib. iii. cap. xiv. Haller, Elem. Phys. t. v. lib. xvi. sect. 2. Zinn. Descrip. Anat. Oculi, cap. iii. sect. iii. Sabatier, Boyer, Charles Bell, Cuvier, &c.

fastened to the bottom of the vessel, the posterior half of the sclerotic having been first removed. With a pair of dissecting forceps in each hand, the choroid coat should be gently torn open and turned down. If the exposed surface be now carefully examined, an experienced eye may perceive that this is not the appearance usually presented by the retina; instead of the blue-white reticulated surface of that membrane, a uniform villous structure, more or less tinged by the black pigment, presents itself. If the extremity of the ivory handle of a dissecting knife be pushed against this surface, a breach is made in it, and a membrane of great delicacy may be separated and turned down in folds over the choroid coat, presenting the most beautiful specimen of a delicate tissue which the human body affords. If a small opening be made in the membrane, and the blunt end of a probe introduced beneath, it may be separated throughout, without being turned down, remaining loose over the retina; in which state, if a small particle of paper or globule of air be introduced under it, it is raised so as to be seen against the light, and is thus displayed to great advantage; or it is sometimes so strong as to support small globules of quicksilver dropped between it and the retina, which renders its membranous nature still more evident. If a few drops of acid be added to the water after the membrane has been separated, it becomes opaque and much firmer, and may thus be preserved for several days, even without being immersed in spirit.

That it is not the nervous layer I detach, is proved by the most superficial examination; first, because it is impossible to separate that part of the retina so as to present the appearance I mention;* and, secondly, because I leave the retina uninjured, and presenting the appearance described by anatomists; especially the yellow spot of Soemmering, which is never seen to advantage until this membrane be removed: and hence it is that that conformation, as well as the fibrous structure of the retina in some animals, becomes better marked from remaining some time in water, by which the membrane I speak of is detached.

* See Haller, Zinn. &c. loc. cit.

The extent and connexions of this membrane are sufficiently explained by saying, that it covers the retina from the optic nerve to the ciliary processes. To enter into further investigation on this subject, would lead to a discussion respecting the structure of the optic nerve, and the termination of the retina anteriorly, to which it is my intention to return at a future period.

The appearance of this part I find to vary in the different classes of animals and in man, according to age and other circumstances. In the fœtus of nine months it is exceedingly delicate, and with difficulty displayed. In youth it is transparent, and scarcely tinged by the black pigment. In the adult it is firmer, and more deeply stained by the pigment, which sometimes adheres to it so closely as to colour it almost as deeply as the choroid coat itself; and to those who have seen it in this state, it must appear extraordinary that it should not have been before observed. In one subject, aged fifty, it possessed so great a degree of strength as to allow me to pass a probe under it, and thus convey the vitreous humour covered by it and the retina from one side of the basin to the other; and in a younger subject I have seen it partially separated from the retina by an effused fluid. In the sheep, ox, horse, or any other individual of the class *mammalia* which I have had an opportunity of examining, it presents the same character as in man, but is not so much tinged by the black pigment, adheres more firmly to the retina, is more uniform in its structure, and presents a more elegant appearance when turned down over the black choroid coat. In the bird it presents a rich yellow-brown tint, and when raised, the blue retina presents itself beneath: in animals of this class, however, it is difficult to separate it to any extent, though I can detach it in small portions. In fishes, the structure of this membrane is peculiar and curious. It has been already described as the medullary layer of the retina by Haller and Cuvier,* but I think incorrectly, as it does not present any of the characters of nervous structure, and the retina is found perfect beneath it. If the sclerotic coat be removed behind, with the choroid coat

* Element. Phys. tom. v. lib. xvi. sect. ii. Cuvier, Leçons d'Anat. Comp. tom. ii. p. 419.

and gland so called, the black pigment is found resting upon, and attached to, a soft, friable, thick, fleecy structure, which can only by detached in small portions, as it breaks when turned down in large quantity. Or if the cornea and iris be removed anteriorly, and the vitreous humour and lens withdrawn, the retina may be pulled from the membrane, which remains attached to the choroid coat, its inner surface not tinged by the black pigment, but presenting a clear white, not unaptly compared by Haller to snow.

Besides being connected to the retina, I find that the membrane is also attached to the choroid coat, apparently by fine cellular substance and vessels; but its connection with the retina being stronger, it generally remains attached to that membrane, though small portions are sometimes pulled off with the choroid coat. From this fact I think it follows, that the accounts hitherto given of the anatomy of these parts are incorrect. The best anatomists* describe the external surface of the retina as being merely in contact with the choroid coat, as the internal with the vitreous humour, but both totally unconnected by cellular membrane or vessels, and even having a fluid secreted between them: some indeed speak loosely and generally of vessels passing from the choroid to the retina; but obviously not from actual observation, as I believe no one has ever seen vessels passing from the one membrane to the other. My observations lead me to conclude, that wherever the different parts of the eye are in contact, they are connected with each other by cellular substance, and, consequently by vessels; for I consider the failure of injections no proof of the want of vascularity in transparent and delicate parts, though some anatomists lay it down as a criterion. Undoubtedly the connexion between these parts is exceedingly delicate, and hence is destroyed by the common method of examining this organ; but I think it is proved in the following way. I have before me the eye of a sheep killed this day, the cornea secured to a piece of wax fastened under water, and the posterior half of

* See Haller, Elem. Phys. tom. v. lib. xvi. sect. ii. Zinn. cap. ii. sect. i. § ii. Boyer, Anat. tom. iv. p. 113. Sabatier, tom. ii. p. 70. Bichat, Anat. Descr. tom. ii. p. 447. Cuvier, Leçons d'Anat. Comp. tom. ii. p. 418. Charles Bell, Anat. vol. iii. p. 51. Ribes, Mem. de la Med. Soc. d'Emulation, tom. viii. p. 635.

the sclerotic coat carefully removed. I thrust the point of the blade of a pair of sharp scissars through the choroid coat into the vitreous humour, to the depth of about an eighth of an inch, and divide all, so as to insulate a square portion of each membrane, leaving the edges free, and consequently no connexion except by surface; yet the choroid does not recede from the membrane I describe, the membrane from the retina, nor the retina from the vitreous humour. I take the end of the portion of choroid in the forceps, turn it half down, and pass a pin through the edge, the weight of which is insufficient to pull it from its connexion. I separate the membrane in like manner; but the retina I can scarcely detach from the vitreous humour, so strong is the connexion. The same fact may be ascertained by making a transverse vertical section of the eye, removing the vitreous humour from the posterior segment, and taking the retina in the forceps, pulling it gently from the choroid, when it will appear beyond a doubt that there is a connexion between them.

Let us contrast this account of the matter with the common one. The retina, a membrane of such delicacy, is described as being extended between the vitreous humour and choroid, from the optic nerve to the ciliary processes, being merely laid between them, without any connexion, and the medullary fibres in contact with a coloured mucus retained in its situation by its consistence alone. This account is totally at variance with the general laws of the animal economy: in no instance have we parts, so dissimilar in nature, in actual contact: wherever contact without connexion exists, each surface is covered by a membrane, from which a fluid is secreted; and wherever parts are united, it is by the medium of cellular membrane, of which serous membrane may be considered as a modification. If the retina be merely in contact with the vitreous humour and choroid, we argue from analogy, that a cavity lined by serous membrane exists both on its internal and external surface; but this is not the fact. In the eye a distinction of parts was necessary, but to accomplish this a serous membrane was not required; it is only demanded where great precision in the motion of parts was indispensable, as in the head, thorax, and abdomen; a single membrane, with the interposition of

cellular substance, answers the purpose here. By this explanation we surmount another difficulty: the unphilosophical idea of the colouring matter being laid on the choroid, and retained in its situation by its viscidity, is discarded; as it follows, if this account be correct, that it is secreted into the interstices of fine cellular membrane here, as it is upon the ciliary processes, back of the iris, and pecten, under the conjunctiva, round the cornea, and in the edge of the membrana nictitans and sheath of the optic nerve in many animals. Dissections are recorded where fluids have been found collected between the choroid and retina, by which the structure of the latter membrane was destroyed. The explanation here given is as sufficient to account for the existence of this fluid, as that which attributes it to the increased secretion of a serous membrane.

I take this opportunity of describing the method I adopt for examining and displaying these and other delicate parts; a method which, though simple, will, I expect, prove an important improvement in the means of scrutinizing the structure of animal and vegetable bodies. I procure a hollow sphere of glass from two to three inches in diameter, about one-fourth of which is cut off at the part where it is open, and the edges ground down so as to fit accurately upon a piece of plate glass, the surface of which is also ground: the object to be examined is attached to a piece of wax fastened upon the plate of glass, and immersed in a basin of water, with the cut sphere, which is inverted over it, of course full of water, and the whole withdrawn from the basin. The part may thus be examined under the most favourable circumstances; it floats in water, the only method by which delicate parts can be unfolded and displayed: the globular form of the vessel answers the purpose of a lens of considerable power and perfection, at the same time that it admits light in any quantity or direction to illuminate the object; and, what is of the utmost importance, a preparation of the greatest delicacy may thus be handed round a class in safety.

Persian Opium.

[From Poulson's American Daily Advertiser.]

An article sold under the name of Persian opium, having been introduced into this city from New York, and sold for opium, which was generally considered as spurious, a number of druggists of this city met at Yohe's Hotel, to take the subject into consideration. A committee was appointed to examine the article, and report to a subsequent meeting. The report having been read, it was ordered that the same should be printed.

The committee appointed to examine the article introduced into some of the drug stores of this city, called Persian Opium, report as follows:—

Of all the articles of the materia medica, opium is the most important, and the most essential to the successful practice of a physician. It is prescribed in a greater range of diseases, than any other medicine, and is an indispensable remedy in many of the most fatal and terrible disorders that afflict the human system. It is not alone to allay the distressing effects of nervous and constitutional irritation, and to alleviate the anguish and torture of pain, that opium is administered; in the crisis of acute disease, life and death are in question, and the result is depending on the judicious administration of a grain of pure opium. It is not necessary here to enlarge on the high obligation imposed on all dealers in drugs to abstain from tampering with the articles they sell, to vend none of whose purity or efficacy they have a doubt; or to dwell on the guilt and criminality of those, who will deliberately adulterate their medicine, or delude the unwary practitioner, by imposing on him a deleterious or inert remedy, to the destruction of his patient.

In the commerce of this country and of Europe, two species of opium are only known, the Turkey and the East India—Persian opium has not before been heard of. The Turkey opium is much more pure than the East India, and is that

which is solely used in the practice of medicine in Europe and America. It is by the strength and qualities of Turkey opium, that practitioners regulate their prescriptions, and it is that consequently, which should alone be sold when opium is to be employed as medicine.

The committee, in order to perform the duty intrusted to them, proceeded to a comparative examination of Turkey opium, and the article called Persian opium, in their sensible, chemical and medical properties; of which they present the ensuing statement.

1st. *Sensible properties*—Persian opium.—Its colour is deep black; soft in consistency, as though lately made up; oily, greasing the paper in which it is enveloped; of a nauseous not well defined smell; of a mawkish taste, with a feeble impression of sweetness. Turkey opium—colour of a reddish brown; when fresh imported, soft, but most generally hard; never oily; of a peculiar virose, well defined smell which cannot be mistaken; of a bitter, strong taste, leaving a sense of pungency and heat on the tongue and fauces.

The sensible properties of these two articles are so entirely distinct, that the most inexperienced person could not hesitate in distinguishing between them. It is the sensible qualities of medicines, that the dealers in them generally consult to ascertain their goodness; and any one with the slightest knowledge of opium, must have suspected the sophistication, or the inertness of Persian opium at the first glance.

2d. *Chemical Properties.*—Half an ounce of Persian opium, so called, was infused in four ounces of water. The whole of it was dissolved, or diffused through the water, leaving no residual matter. When filtered, the infusion was *black*, and there was collected on the filter 37 grains of a black matter, having a faint smell of tobacco.

The same quantity of Turkey opium was infused in the same quantity of water. The soluble parts were soon dissolved, leaving behind an elastic, plastic mass, having a strong resemblance to gluten, and which, when dried, weighed 71 grains. The solution was of a *light wine colour.*

To a portion of the watery solution of Persian opium, was added a small quantity of alcohol. A precipitation of a dirty

white colour, diffused through the solution, immediately took place. The same quantity of the watery solution of Turkey opium, and the same quantity of alcohol, as in the preceding experiment, were mixed together. The solution remained perfectly transparent.

To ascertain the effects of different tests, the watery solution of the spurious and Turkey opiums was used, the result of which is contained in the following experiments:

Infusion of galls, Persian opium.—A slight yellowish white coloured precipitate that did not subside. Turkey opium—a copious white precipitation, that immediately subsided with an appearance of being curdled.

Acetate of lead, Persian opium.—A slight precipitation of a brown colour, diffused through the solution. Turkey opium —white precipitate soon subsiding.

Sulphate of copper, Persian opium—No immediate change, dark coloured precipitate on standing. Turkey opium—instant precipitate of a white colour.

Sulphate of zinc, Persian opium.—No precipitate. Turkey opium—a copious white precipitate.

Subcarbonate of potash, Persian opium.—No precipitation —the solution becoming slightly turbid, of a dark colour. Turkey opium—A copious curdled precipitate, of a white colour.

Sulphate of iron, Persian opium.—No precipitate—the solution became of a deeper colour. Turkey opium—a reddish brown precipitate, which did not subside.

Half an ounce of Persian opium was infused in four ounces of alcohol. There remained 67 grains insoluble residue. A portion was evaporated to dryness, and a soft sweetish extract was obtained without any resin. To the alcoholic solution water was added, a diffused precipitation ensued, which subsided on standing some hours. The addition of water to the alcoholic solution of Turkey opium, occasioned an immediate copious precipitate subsiding at once.

Having made the above general examination of the chemical qualities of the article submitted to our consideration, it was thought proper to ascertain what were the effects produced by it on the system.

3d. *Medical properties.* A grain was taken by one of the committee. The pulse beat 85 strokes in a minute. No perceptible alteration in it took place. About an hour afterwards, a slight nausea was felt. Another of the committee took 50 drops of the tincture without perceiving any effect. Mr. Britt, a student of medicine, took two grains at a dose, in the morning, two hours after breakfast, his pulse beating 80 strokes. In 5 min. 78—10 min. 76—15 min. 76—25 min. 75—30 min. 76—40 min. 74. Two grains more were taken—5 min. pulse 72—15 min. 72—25 min. 72—35 min. 72. The skin remained perfectly cool; there was no sensation of heat, flushing of the face, or other effects produced by a large dose of opium. An hour and a half after taking the dose, a slight nausea was felt, and about half a gill of fluid thrown from the stomach. One of the students at the hospital took half an ounce of the tincture in one dose, without experiencing any stimulant or soporific effect from it.

From the examination to which the committee have submitted the article called Persian opium, they feel a perfect confidence in pronouncing it to be devoid of the properties of opium; and give it as their opinion, that it cannot be relied on to produce the effects of opium; but, that its administration for that purpose, in critical cases, must be attended with the greatest danger to the life of the patient.

SAMUEL JACKSON,
EDWARD LOWBER,
THOMAS WILTBERGER.

Philadelphia, July, 1820.

SELECTED REVIEWS.

Report from the Select Committee on the Doctrine of Contagion in the Plague, pp. 102. Ordered by the House of Commons to be printed, June 14, 1819.

[From the Edinburgh Medical and Surgical Journal, for Jan., 1820.]

Our readers will probably recollect, that, in the course of the last session of Parliament, Sir John Jackson moved for a committee to inquire into the expediency of abrogating or modifying the restrictions imposed by the quarantine laws; in which motion he was supported by the Right Honourable F. Robinson, President of the Board of Trade; and the reason assigned for this measure by Sir John Jackson was, that the doctrine of contagion had been shaken by evidence which came before a committee during the preceding Parliament, to inquire into the epidemic fever then prevailing in the metropolis. The principal objects of enquiry on the subjects in question were, *first*, Is the plague capable of being communicated from person to person, either by immediate contact with those diseased, or, intermediately, by contact with infected goods? Or, *secondly*, Is it an epidemic depending only on a peculiar state of atmosphere? We shall now give the report in full, which is the result of their labours.

"Your committee being appointed to consider the validity of the received doctrines, concerning the nature of contagious and infectious diseases, as distinguished from other epidemics, have proceeded to examine a number of medical gentlemen, whose practical experience, or general knowledge of the subject, appeared to your committee most likely to furnish the means of acquiring the most satisfactory information. They have also had the evidence of a number of persons, whose residence in infected countries, or whose commercial or official employments, enabled them to communicate information as to

facts, and on the principle and efficacy of the laws of quarantine. All the opinions of the medical men whom your committee have examined, with the exception of two, are in favour of the received doctrine, that the plague is a disease communicable by contact only, and different in that respect from epidemic fever; nor do your committee see any thing in the rest of the evidence they have collected, which would induce them to dissent from that opinion. It appears from some of the evidence, that the extension and virulence of the disorder is considerably modified by atmospheric influence; and a doubt has prevailed, whether, under any circumstances, the disease could be received and propagated in the climate of Britain. No fact whatever has been stated to shew, that any instance of the disorder has occurred, or that it has ever been known to have been brought into the Lazarettos for many years: but your committee do not think themselves warranted to infer from thence, that the disease cannot exist in England; because, in the first place, a disease resembling, in most respects, the plague, is well known to have prevailed here, in many periods of our history, particularly in 1665 and 1666. And further, it appears, that in many places, and in climates of various nature, the plague has prevailed after intervals of very considerable duration. Your committee would also observe, down to the year 1800, regulations were adopted, which must have had the effect of preventing goods, infected with the plague, from being shipped directly for Britain; and they abstain from giving any opinion on the nature and application of the quarantine regulations, as not falling within the scope of inquiry to which they have been directed, but they see no reason to question the validity of the principles on which such regulations appear to have been adopted."

We shall defer our remarks on this report until we have laid before our readers, as clear an abstract of the evidence before the committee as our limits will allow.

Dr. Charles Maclean, who was first examined, states, that he has studied the plague in the Greek Pest-Hospital at Constantinople, in which he resided seventeen days, and that, during his stay there, he himself caught the disease. The inferences supplied by his experiments on plague were sufficient

to confirm him in those conclusions against the existence of contagion, which he had previously deduced in respect to yellow fever, typhus, and epidemic diseases generally.

After this sweeping conclusion, he thus modestly speaks of himself, and of his search after truth:

"If it should be thought that this is but an inadequate experience, I would entreat of the committee to recollect how little the value of experience may be commensurate with its duration; and that the result of experience in the application of false knowledge, is but dexterity in the practice of error, which is as much worse than mere inexperience, as false knowledge is worse than absolute ignorance. Since the establishment of the doctrine of contagion, I am not aware that any other physician, now living, has been experimentally in such intimate collision as myself with what is called the plague of the Levant;* and if the result of my researches should ultimately prove adequate to dispel, in respect to the cause of that malady, the darkness of centuries, they will not be denied to be of unprecedented efficiency."

He then goes on to assert, that the plague is not contagious, and being asked to explain how he caught the fever when in the Pest-Hospital, he replied, "by the air."

"It was in August, a month in which plague generally prevails most at Constantinople; I was a stranger to the climate and otherwise under particular circumstances; the deprivation of food, insufficiency of nourishment, and also some degree of irritation of mind, under the idea, that the people around me were endeavouring to frustrate my object; and all these

* We do not exactly understand the force of this assertion. How many physicians may be now living, who have been in as intimate collision with what is called the Plague of the Levant as Dr. Maclean, we do not know; and if none of them placed themselves in such a situation *experimentally*, we know that some did it *voluntarily*, and from the noblest motives that can influence human conduct. During our expedition to Egypt, the present President of the College of Physicians of Edinburgh, three several times, shut himself up in the lazarettos for months together, when the inferior officers to whom the duty was allotted were themselves infected, and rendered incapable of attending the sick. Nor did he escape with impunity, for he was twice affected with the disease, certainly not from the causes to which Dr. Maclean has ascribed his own illness.

might surely be sufficient to account for the occurrence of disease during the pestilential season, without supposing contagion."

Dr. Maclean, in short, considers, that his having been affected with plague, must be attributed to the climate, short commons, and his being put out of humour; and that his having daily and hourly contact with patients labouring under an aggravated form of that disease, and living in the contaminated air of the hospital, had no share in its production.

Dr. Maclean distinguishes contagious from epidemic diseases, by supposing that the former are capable of being communicated by contact, or contiguity, whereas epidemics are produced by such causes as are capable of operating simultaneously upon any portion, or the whole of a community, and that they cannot become contagious. He adds, that contagious diseases occur only once during life; but on being asked if the small pox be not a contagious disease, and whether it has not occurred more than once in the same person, he admits that it is contagious, but does not believe it ever has twice affected the same person; and, were he to see a case of second small pox, he would distrust even the evidence of his own eyes, p. 13.* In defining the plague, he says; its phenomena embrace almost all the symptoms which the living body is capable of exhibiting, few of these, however, occurring at the same time, in the same person. He quotes Fra Louigi di Pavia, who attended a plague hospital for 30 years, and who says, the surest symptoms of plague are the eyes dusky and turbid, fixed and sparkling; the tongue forked, of a whitish colour, inclining to yellow, with the extremity red, which branches out in a number of small ramifications, exceedingly inflamed. He proceeds to describe, from his own experience, the appearance of the countenance:

"The skin and muscles of the face exhibit a tremulous appearance, and with a certain fierceness of the eyes, and sometimes an involuntary motion of one of the eye-lids, as in winking, gives an expression of rather a ludicrous wildness of

* Syphilis also is a contagious disease, and nobody doubts that it can occur *frequently* during life.

aspect, increased by the efforts of the patient to preserve the composure of his features, as if conscious of the insubordination of his features to preserve their composure."

The appearance of the tongue varies from blackish to brown, white, and now and then flesh-red colour; he has also seen it of a glossy grey, *resembling the first formation of icicles upon water.* And this last state he considers as promising the most favourable termination. The brain and circulating system are variously affected in the earlier and milder stages of the plague. There is a rapid succession of ideas, hurried speech, a tremulous and unsteady walk, and a distracted look. The higher degrees are accompanied by delirium, anxiety, languor, and melancholy, as well as great muscular debility, throughout the disease. At the commencement, the bowels are usually constipated; but, in the progress of the disease, diarrhœa and hæmorrhage are not unfrequent symptoms; and when severe, denote much danger. Dr. M. adds, however, that none of those symptoms are pathognomic, nor does he consider them as very distinct from those of the severe form of typhus and yellow fever in other countries. He does not put much dependence on the glandular affections as diagnostic of plague, but thinks the carbuncles are more decided marks of the disease, for buboes, he asserts, occur in typhus.* If these observations are correct, the inference, we think, must be, either that every fever is plague, or that nothing is plague.

During the prevalence of the plague at Constantinople, the Frank merchants shut themselves up in their houses, and, by so doing, they generally avoid the disease. But Dr. Maclean attributes the success of this measure to their avoiding vicissitudes of atmosphere, by keeping their windows shut during the pestilential season. Speaking of the experiments of Dr. White, he says, that gentleman first rubbed on his arm matter which was taken from a bubo of a person supposed to have the plague, by which he was not affected; he afterwards inoculated himself thrice in the arm with similar matter: that he was not affected by the two first inoculations, but, after the

* Buboes occur *very rarely* in typhus, and only as sequelæ of the disease; they occur *very commonly* in plague, and always as a primary symptom.

third, he sickened of the plague, and died: which circumstance, nevertheless, Dr. Maclean does not attribute to the experiments previously made by Dr. White, but considers as an accidental coincidence his being seized with the disease at that time.

On being asked his opinion on the utility of the quarantine regulations, he replies, that he does not consider the establishment of lazarettos of the smallest use, and he goes on to say,

"If it be true, that there never has arrived at any one period of time any one person from the Levant, or any other place, actually labouring under the plague; and if it be true, according to the advocates for contagion, that goods, wares, and merchandize, can retain infection for seven, fourteen, or twenty years; it must be apparent, that with respect to goods, as well as with respect to persons, a quarantine of forty days can be of no sort of use."

He states, on the authority of Dr. Carunhanha, and of Dr. Grieves, superintendent of quarantine at Malta, that no case of plague had occurred in the lazaretto for fifteen years; nor, in the plague of 1813, was any officer of the lazaretto affected by the disease. On being asked, if he was aware, that, in the towns of the East, the Turks suffer in a greater proportion than the Christian population? he answers, that he knows the fact to have been so represented by Christian travellers in the Levant; but that he considers this as a mere assumption; such travellers cannot possibly know any thing at all of the matter, except what is represented to them by Christians believing in contagion, or by the superintendents of plague establishments; and that the want of bills of mortality amongst the Turks must render such inferences of no value. Yet, in his very next answer, he asserts, that the Turks, who have no belief in contagion, recover in much greater numbers from the plague than the Christians who entertain that belief; and this, he adds, is "confirmed by facts, and it is stated by various travellers."

From the above statement, it is sufficiently evident, that Dr. Maclean asserts or denies, as he may find most convenient; for here he has first denied, and then believed, the statements

of travellers; first stated the want of bills of mortality to be an obstacle in ascertaining the proportion of recoveries, and then has done it quite as well without them. The doctrine of contagion, according to Dr. Maclean, originated in the council of Trent: the pope, desirous of removing the council to Bologna, having suborned Fracastorius and certain other physicians to circulate the report, that a malady had broken out, which spread itself rapidly from person to person by contact: and that the ancients had no idea of contagion. We have the following reasons given for believing that the plague is not contagious:

"1st, That this and all other epidemic diseases appear generally at certain seasons, and disappear at other certain periods, different in different countries. 2dly, They also cease generally at the time at which the greatest number of persons are affected, which seems wholly incompatible with the existence of contagion. And, 3d, Because they are capable of affecting the same person repeatedly, which there is no proof that contagious general diseases are capable of doing."

The epidemic season at Smyrna, as stated by Dr. Maclean, is from March to June or July; at Constantinople, it begins in July or August, and ends in November or December;—and, if this be correct, we have thus two places, not far apart, the epidemic seasons of which, taken together, occupy ten months in the year. But it will be found, in the progress of this subject, that Constantinople is never totally free from plague.

This is the substance of Dr. Maclean's evidence, which we have been tempted to give thus much at large, because he is the Goliah of the non-contagionists; but, to avoid going over the same ground again, we shall reserve our remarks until we have gone through the remainder of the evidence.

The next person examined was Dr. Thomas Forster, whose answers were to the following effect:—That he has not any personal knowledge of the plague; but from such information as he has been able to collect, he considers it a contagious disease, rendered less so by ventilation; he believes buboes and carbuncles to be the best diagnostics of this malady; he thinks the difference of our climate the principal cause of our exemption from plague, but does not regard our long immunity from

plague as sufficient security against its recurrence if imported, since there might exist a state of climate and other causes to favour its production; he conceives that epidemics depend much upon a peculiar electric state of atmosphere. With regard to the possibility of an epidemic attacking the same person more than once, he says, that he is acquainted with a person who has had small pox three times; he thinks Lucretius's opinions of the contagious nature of certain diseases as deserving to be noticed on the minutes of committee; and these we shall give presently in some extracts from the sixth book of his poem, De Rerum Natura.

Dr. James Johnson had served four or five years in the Mediterranean, where, although he had not an opportunity of seeing the plague, yet he acquired information respecting its nature. He is of opinion that it is contagious, and that its activity depends much on a particular state of the atmosphere; that infected goods or merchandize may communicate the disease, but more particularly the clothes of diseased persons. He entertains this opinion on the authority of Dr. Russell, and other authors who have described the disease; and the circumstance of its being an eruptive disease, strengthens that belief, as other diseases of that character universally arise from specific contagions or poisons. The malignant fevers of Trincomalee, Batavia, and Diamond Harbour, are not analogous to plague. *Epidemic diseases* may at first be produced by atmospheric influence, and afterwards be propagated by contagion. He thinks the return of plague is a revival of infection that has been latent or dormant, until a particular state of atmosphere rouses it to action. Dr. Johnson well remarks, that the non-occurrence of plague in a lazaretto, for fifty or an hundred years, is not a sufficient reason for doing away the quarantine laws; for although the climate of this country may be unfavourable to the production of plague, yet we do not know what share quarantine regulations may have had in our exemption from that disease.

Dr. William Gladstone, when at Constantinople in 1806-7, saw cases of the plague in the months of December, January, February and March. The origin of the disease he takes to be a diseased constitution of atmosphere, and deficient ventila-

tion of the houses; and he attributes the plague of London in 1665 to similar causes. The absence of plague in this country he thinks cannot be attributed to our quarantine regulations, as they are very imperfect; and he does not suppose the contagion of plague ever to have been imported into the lazarettos, as there is not any instance of the expurgators having been affected. He adds, that epidemics may certainly become contagious.

Dr. A. B. Granville, who had seen the plague in Turkey, Greece, Asia, Syria, Egypt, &c. and in Constantinople, where he resided two years, describes the symptoms to be pretty generally these: Dizziness, great pain in the head, great prostration of strength, the nervous system very particularly affected, occasionally sickness of the stomach, and, invariably, the appearance of glandular swellings, with carbuncles, and other local or cutaneous symptoms, particularly livid spots on the body, if the disease goes beyond sixty hours. There are no symptoms of inflammation whatever, none, at least, such as attend inflammatory diseases during their first attack. He is ignorant of the cause, and only knows that the disease exists, and is spread by contagion. In Constantinople, Smyrna, Egypt, and Syria, the Franks, who can afford it, shut themselves up during the plague, and he never heard an instance of such persons being infected where the seclusion was perfect. Of course, this separation could not avail, if the disease was communicated by the air. In Egypt, this separation is continued until St. John's day, June 24th, when the dew falls. Dr. G. considers that the state of the air has no effect in checking the disease, except, perhaps, that it lessens the liability of persons to be affected by it. He understands that the plague has been carried from Aleppo, by caravans, to Bagdat, and Damascus: to the latter place it was carried by the army of some Pashwa, who had been on the coast to assist in the reduction of Jean d'Acre; that it has likewise been carried over land westward from Constantinople to Adrianople.

Dr. Granville distinguishes contagion from infection, confining the former term to the communication of disease, by mere contact with a diseased person, or with something that has been in contact with him, whilst he limits the latter to re-

ceiving the disease through a contaminated atmosphere in a sick chamber. In support of this distinction, he mentions the following occurrence, which we shall give in his own words, as it bears directly on the object of the committee, and is very remarkable. "There is one instance in point amongst the most recent, and it rests on the highest authority." During the plague at Corfu, in 1815,

"One of the villages, which had been infected several months, had for some time, I believe 48 days, exhibited no sign of the plague, owing to the measure of segregation adopted by Sir Thomas Maitland. The village was reported to be released, and fumigation preparatory to its receiving pratique ordered. The officers who had the surveillance of the village during the three or four months, had resided in the church, from there being no house that was not thought to be infected, in which church the priest and the people had been crowded just before the laws of segregation were ordered by Sir Thomas Maitland. Some of these died subsequently, for the church was ordered to be shut the instant the plague began. It was therefore necessary to purify this edifice before the people could go in again, and leave being granted for this purpose, the priest went in, and having touched the cloth of the great altar, so as to purify it by shaking it, he was seized with the plague, beginning with the headache, of such violence, that he fell on the steps of the altar, almost immediately; and in three hours, before he could be carried to the lazaretto, he expired, with buboes under the arm, and livid spots over the body."

Dr. Granville expresses his belief, that the poison, in this instance, was conveyed by touch alone, since the officers who had resided two months in this church were not affected. Since 1814, he says cases have occurred in the lazaretto at Leghorn, where persons touching the infected part of bales of merchandize have been seized with plague. At Marseilles, a similar circumstance has happened twice in fifteen years, and, recently, according to the dispatches of Mr. Hopner, the British Consul at Venice, in October 1818. During the prevalence of plague in the Levant, goods in general are not allowed to be shipped for England, and many vessels perform quarantine at ports in

the Mediterranean, which lessens the probability of the contagion of plague having ever been imported into our lazarettos: But the fact of the plague never having taken place here, for 154 years, or if for six or seven centuries, would not give the hope that it *cannot* exist in a British atmosphere, since we know it existed here in 1665.

John Green, Esq. is treasurer to the Levant Company, and resided in Constantinople from 1774 to 1780. In the year 1778, he witnessed a violent plague during the months of May, June and July, which destroyed 200,000 persons. The most important part of his testimony, is that which expresses his disbelief, that goods cannot convey the contagion of plague; in support of which opinion, he mentions, that the clothes and bedding of persons dying of plague are commonly sold, and used without fear.

He states, that the disease may attack the same person frequently; that he knew an Abbé, of the French hospital, who acknowledged he had had it ten or twelve times. He has heard, however, that there is a particular kind of bubo called "*the Blessed*," which, when it occurs, renders the person much less liable to have the plague again, or if he should be afterwards infected, the disease is slight. Mr. Green conceives, that the dew which attends the hottest seasons has a powerful influence in checking the plague; and five years ago, Mr. Morier informed him, that the plague which had begun to be prevalent, we believe, at Alexandria, ceased suddenly on the coming on of dense fogs, which fogs prevailed also in this country at the same time. The inhabitants of Constantinople believe, that exposing infected clothes for one night to the dew, has more effect in purification, than a similar exposure for a whole week in the sun. This last part, however, shows that apprehension does exist as to danger from this source. He would not recommend the abolition of quarantine laws, as the state of health of crews ought to be inquired into; and if the quarantine establishments of this country were abolished, no matter why, it might occasion the exclusion of our vessels from other countries.

"The lazarettos of England are old men of war, with houses built upon them like an ark. The sides of these houses

are open like a brewhouse, with shutters, and the floors are all open gratings in fact, so that the ventilation is excessive on board these vessels in Standgate creek, greater than it is possible to give on any building on shore. The ships also swing with the tide; that is, when the tide turns, they change their sides to windward every six hours."

The mode of ventilation is different for different kinds of goods. Some articles, as goat's wool, if brought in a ship with a foul bill of health, are ordered to be entirely turned out of the packages, and exposed to the air. Bale goods are slit open on one side, and exposed for some time, which is then sewed up, and the other side exposed. The expurgators are ordered to thrust their arms into the bales as far as possible.

In Dr. M'Leod's evidence, nothing important is added to what we have already stated.

Sir A. B. Faulkner was staff physician to the forces at Malta, during the plague which occurred there in 1813. He considers that this disease is propagated by contact only, or close approximation to the person or thing infected. He gives numerous and decided proofs of the success which attends complete separation of the healthy from the diseased, and instances the public general hospital, public prison, one of the barracks, and all the convents, except one, and the introduction of the disease into this last could be accounted for. He did not perceive that changes of temperature had any effect upon the progress of the disease, but he observed, that a high wind, from whatever quarter it blew, was always accompanied with an increase of the numbers affected. The disease had certainly no connection with marsh miasmata, the part of the town the most subject to marsh fevers, being less affected by plague than many others which were considered more healthy. In the treatment of the plague, camphor, calomel, and topical blood-letting by leeches, were tried, but the remedies found to be of the most service, were cold affusion and turpentine.

Dr. Pym has no doubt that the plague is contagious, but not very actively so. He considers the fever which prevailed at Gibraltar in 1804, and which destroyed 6000 persons, out of a population of 20,000, to have been a more formidable disease than the plague. He thinks it probable, that the contagion of

plague has never been brought into our lazarettos, which he thinks may be accounted for, upon the supposition that the cargoes are packed by healthy persons, or at a time when plague does not prevail. Before 1813, however, the duties of the expurgators were negligently performed.

Sir James M'Grigor was at the head of the medical staff of the British army which came from India up the Red sea, and disembarked at Kossier in May 1801. This army was 7886 strong, of which 3759 were Europeans, and 4127 natives of India; there died of the plague 38 Europeans, and 127 natives of India, in all 165. The disease came on with fever, and buboes; the two first cases could not be closely traced, but the sufferers had been at Rosetta, where there were cases of plague; from these two, four more caught the disease, and all the six died. The disorder is contagious, but he has known instances of contact without any ill effect. There were many instances of its spreading by contagion, particularly one of a prisoner confined in the guard-house, who afterwards appeared to have the plague, which was thus communicated to the other prisoners, and the men on guard. The plague did not appear connected with miasmata, as it was met with in the progress of the army through Upper Egypt. And Sir James M'Grigor thinks it probable, that it always exists in some degree in Egypt.

Dr. James Curry considers the contagious nature of plague to be clearly shown by the order of its progress, and the protection afforded by segregation. He supposes it may be often generated anew; but that it requires for its existence a peculiar state of atmosphere, which he thinks greatly to depend upon electric influence. He believes it was first introduced into the Mediterranean by fomites brought from Jerusalem and Syria by the Crusades.

Dr. Tainsh, surgeon of the Theseus, was in the Mediterranean in 1798, 9. He received on board his ship five cases of plague, some of them severe. One of them, that of a Frenchman, had suppurating buboes, but recovered. These cases have been alluded to by Dr. Pym. The disease appears to have been communicated by intercourse between two vessels at sea;

but the circumstances were not inquired into by the committee, and the case is, therefore, rendered of no value.

Mr. Edward Hayes, who is, we believe, a merchant, resided 44 years in Smyrna; during which time he frequently saw the plague. He speaks confidently of the security afforded by confinement within doors during its prevalence. At Smyrna, the disease generally begins in March or April, and ends in August at the latest; but great cold or heat commonly arrest it. He informs us, that goods are shipped at all times, whether the plague prevails or not; that the merchandize may, therefore, be infected; but that the time occupied by the voyage, and the state of the persons exposed to the contagion, are probably causes which prevent its taking effect in this country and many other places; that, abroad, the plague does at times affect the Lazarettos. The quarantine regulations in Holland do not permit any vessel from the Levant to enter, if the plague prevailed there at the time of her departure. He affirms, that plague is communicable by goods; all woollen and linen goods may infect; animal substances, particularly goat's wool, are more dangerous than vegetable. He has known the disease communicated by inoculation, but is not aware that vaccination has been tried as a security against plague. He adds, that the Turks have lately learnt the danger of preserving the clothes of persons dying of this disease.

The evidence of Sir Robert Wilson is only a repetition of what has been already stated by Sir James M'Grigor. Dr. Nevinson, Dr. Powel, Dr. Frampton, and Dr. Ash, profess not to have seen any cases of the plague, but they all agree in believing that it is contagious, both as regards persons and merchandize. The evidence of Dr. Latham is to the same effect; and, in the examination of this gentleman, a strong leaning of the examinants to one side of the question, which we think has been rather injurious to the whole of this investigation, is more than usually conspicuous.

Dr. Frank saw frequent cases of plague, whilst with Sir Ralph Abercrombie, in the year 1800, at Aboukir. He could distinctly trace its progress, by contagion, from one person to the sick and hospital attendants, who afterwards became infected; whilst none of the wounded, who were in a separate

hospital, about half a mile from the former, had the disease. He has seen several instances of contact with impunity, and has himself handled plague patients. His opinion is, that the plague of London in 1665 was imported, and that circumstances might arise in this country favourable to its existing again.

Dr. Harrison has not any doubt of the highly contagious nature of plague; he believes it to be typhus, with the ordinary symptoms more severe, and with the addition of broad purple spots, buboes, and carbuncles. He was at Naples whilst the plague raged, in 1816, at Noya, 150 miles from thence; and, in 1817, there was a contagious and very mortal fever at Naples, supposed to be the plague. Dr. Harrison is of opinion that the quarantine laws are useful, but that they might be modified as to persons; and he mentions a fact, which is very important,—that while passengers, who have made a long voyage, are liable to perform quarantine, couriers, who come in the least possible time, are not under such restrictions. The next person examined was Mr. John Jenkins, a pilot of the port of Liverpool, who says, that, in eleven years, he never knew any one ill on board of the lazaretto, and that there is a surgeon who attends every day.

We come, lastly, to Dr. John Mitchell, whose opinions respecting contagion are precisely those of Dr. Maclean; for which reason, and because a large part of his evidence is founded more upon the opinions of others than his own observation, our abstract of his testimony will be short. He tells us, that Desgenettes and Larrey, the chief physician and surgeon of the French army, exposed themselves to the plague, without taking the disease; and Desgenettes showed Assalini two punctures which he had made on himself, without effect, with a lancet dipped in the pus of a plague bubo. M. Assalini was told by Citizen Martin, captain of the Lazaretto at Marseilles, who for thirty years held that post; that, during that time, he had seen emptied some millions of bales of cotton, silk, fur, feathers, and other goods, coming from several places where the plague raged, without having seen any accident therefrom to the persons employed. Dr. Mitchell classes the plague with malignant fevers, and mentions that Assalini does the same. Dr. Mitchell

lays much stress upon his experience when resident clerk in the Infirmary of this place; from which he concluded that the fever he then saw was not contagious. His opinion, promulgated in his inaugural dissertation, was at that time almost universally considered as heterodox. At any rate, it is completely overthrown by the experience of the late epidemic; during which a succession of matrons, nurses, and resident clerks, in both hospitals, have been infected, with scarcely an exception.

Dr. Maclean comes again before the committee, and this second testimony concludes their volume. He amends his definitions of epidemics, contagion, and infection, and adds fourteen reasons more to those already given for his want of faith in the doctrine of contagion. But we have neither room nor inclination to go deeper into the discussion of his views.

We have now furnished our readers with the substance of the examinations before the Committee of the House of Commons; and that our abstract is not more clear, has arisen from the very desultory manner in which the inquiry was conducted. The number of medical men examined was nineteen; only two of whom, Dr. Maclean and Dr. Mitchell, deny the contagious nature of the plague. The non-contagionists are, consequently, to the contagionists as ten to one. But we feel confident that, in point of fact, the disproportion generally is much greater; and if the committee had extended their examination to the whole of the faculty of London, it is our conviction they could not have raised more than one recruit for Dr. Maclean, perhaps not one; for we do not believe that Dr. Robarts, who, it appears, first made infidels of the right honourable President of the Board of Trade, and the honourable Chairman Sir John Jackson, is at all prepared to go the same length with Drs. Maclean and Mitchell in this heresy.

That the plague is highly contagious is, perhaps, the only inference that can be much relied upon, which results from the labours of the committee. The conflicting assertions as to facts, and the various opinions deduced from similar facts, are, at first sight, not calculated to leave a favourable impression as to the value of medical science; but the numberless sources of error which perplex such investigations, are only known to those

who have long applied to them. For our opinion on the general question, Whether typhus and other fevers are contagious? we shall refer to former numbers of our Journal.* That contagion was a lamentable feature in the epidemic fevers which have recently afflicted Britain and Ireland, is so evident, that we cannot comprehend the organization of that man's mind who doubts, and still less his who denies it.

With regard to the frequent allusions, in the evidence which we have just examined, to the opinions of the ancients on contagion, we do not think them of much value; for it is not sufficient to know that they believed in contagion, unless we are in possession of their reasons, which we cannot know while we are ignorant of the diseases they describe: For example, Whether the plague of Athens, described by Thucydides, was the disease now called plague, is very doubtful; we think it more probable the plague of the present day was then unknown, as Herodotus, who was the contemporary of Thucydides, makes no mention of the plague when describing Egypt, though he has said much of the climate; and mentions, that, at particular seasons, the Egyptians were habituated to taking emetics and cathartics for three successive days, in each of the months they considered the most unhealthy; and it is not to be supposed that so accurate an historian could omit so remarkable a feature as the plague has since formed in the natural history of that country. Lucretius, who wrote about 350 years later, and who was in those days what Armstrong the poet has been in ours, has powerfully and beautifully described a pestilence, from which we are tempted to give the following short extracts, to show that, 1500 years before the council of Trent, in addition to a knowledge of endemics, and epidemics, contagion was allowed to have its share in the spreading of the disease.

> "Nam quid Britannis cœlum differre putamus,
> Et quod in Ægypto est, qua mundi claudicat axis?
> Quidve quod in Ponto est, differre et Gadibus, atque
> Usque ad nigra virum, percocto sæcla calore?"
>
> LUCRET. *de Rerum Natura*, Lib. vi. l. 1104.

* See our Reviews of *Dr. Barker's Report*, &c. in our 58th Number, and of *Dr. Stokes on Contagion*, in No. 59.

"Est Elephas morbus, qui propter flumina Nili
Gignitur Ægypto in media, neque præterea unquam."
LUCRET. *de Rerum Natura*, Lib. vi. l. 1112.

"Et, cum spiranteis mistas hinc ducimus auras
Illa quoque in corpus pariter sorbere necesse est."
l. 1127.

"Quippe et enim nullo cessabant tempore apisci
Ex aliis alios avidi contagia morbi."
l. 1234.

"Qui fuerant autem præsto, contagibus ibant
Atque labore." l. 1241.

The belief that the plague is contagious, is not in the least invalidated by the fact that many persons have escaped after an exposure that might have been expected to produce infection, since the same may be urged against small pox, which Dr. Maclean admits to be a contagious disease; and he must also admit, that, whilst many who are exposed to that contagion contract the disease, there are likewise many who do not; a remark which applies also to inoculation in both. The question has been repeatedly asked, Whether our long immunity from plague were sufficient ground for abrogating the quarantine laws? But it will be seen, that, although the island of Malta is, from many causes, much more exposed to this infection than Britain, yet it was 138 years free from plague, and we have exceeded that period only sixteen years. The danger of altogether abolishing quarantine regulations is great and obvious; but, at the same time, we have no doubt they might be amended, and rendered less inconvenient to commerce.

We shall conclude by giving a table, which will place in a forcible light the contrariety of opinion amongst the persons examined by the Committee.

	If contagious.	If contagious by Goods.	If Quarantine is of use.	If an eruptive Disease.	If the Plague of 1665 were true Levant Plague.	If from personal Observation.
Sir James M'Grigor	Yes	Yes	Yes	Yes	Yes	Yes
Sir B. Faulkner	Yes	Yes	Yes	Yes	Yes	Yes
Dr. Pym	Yes	Yes	Yes	Yes	Yes	No
Dr. Granville	Yes	Yes	Yes	Always	Yes	Yes
Dr. Powell	Yes	Yes	Yes			No
Dr. Ash	Yes	Yes	Yes			No
Dr. Latham	Yes	Yes	Yes			No
Dr. Frank	Yes	Yes	Yes			Yes
Dr. Frampton	Yes		Yes	Yes		No
Dr. Harrison	Yes	Yes			Yes	No
Mr. Hayes	Yes	Yes				Yes
Dr. Tainsh	Yes			Yes		Yes
Sir R. Wilson	Yes					Yes
Dr. Johnson	Yes	Yes	Yes	Always	Doubtful	No
Dr. Forster	Yes	Yes	Yes	Yes	No	No
Dr. Gladstone	Yes	Yes	Yes	Generally	No	Yes
Dr. Nevinson	Yes	Yes	Yes	No	Yes	No
Dr. M'Leod	Yes	Yes	Doubtful	Yes	Yes	No
Dr. Curry	Yes	Improbable	Yes		Yes	No
Mr. Green	Yes	No	Yes	Yes	Yes	Yes
Dr. Maclean	No	No	No		No	Yes
Dr. Mitchell	No	No	No	No	No	No

Further observations on the Internal Use of the Hydrocyanic (Prussic) Acid in Pulmonary Complaints, Chronic Catarrhs, Spasmodic Coughs, Asthma, Hooping-Cough, and some other Diseases. With full directions for the Preparation and Administration of that Medicine. By A. B. GRANVILLE, M. D. F. R. S. F. L. S. M. R. I. Physician in ordinary to His Royal Highness the Duke of Clarence; Licentiate of the Royal College of Physicians of London; and Physician-Accoucheur to the Westminster General Dispensary. London, 1819. Octavo, pp. 82.

[From the Medico-Chirurgical Journal, for October, 1819.]

DR. GRANVILLE, after detailing the modes of preparing, and the physical properties of, the prussic acid, prefaces his account of its effects upon the human frame by remarking, that though poisonous in itself, like many of our best remedies, it may be used with perfect safety and advantage when properly administered; and, that no case has yet been recorded in which it has proved either fatal or injurious.

Another objection which he admits to be better founded, viz. that it has failed in producing any good effect in some of the complaints in which it has been exhibited, the author thinks only goes to prove a want of sufficient knowledge in the person employing it; but, we would ask, may it not also, peradventure, prove the incompetency of the medicine employed? The following are the effects of this remedy as stated by Dr. Granville:

"The prussic acid is evidently sedative, more so even than opium; but its specific mode of action is somewhat different, both as to its progress and effect, from that of the latter substance. The prussic acid, when administered to a patient exhausted by disease, and by the means employed to cure it, appears to exert an immediate influence upon the nervous system; it gradually diminishes all irritability, checks a too rapid circulation, and calms many of the symptoms of fever. If a dry cough be present, it promotes expectoration in the first instance, and subsequently stops the cough itself. The spirits, before exalted, soon feel the quieting impression of the acid; they become subdued; the speech, countenance, even the expression of the eyes assume a character of unusual meekness; there is a relief from pain and actual suffering; the patient feels it, and is grateful: sleep comes on undisturbed, respiration is soft, and the pulse more quiet than at other periods of the complaint, having lost the thumping beat of irritation. In some few cases these sedative effects are so much more considerable, that the patient expresses that he feels himself as if only *half alive*. On these occasions there is an *apparent* entire prostration of strength, great lowness of spirits, and unwillingness to move, speak, or take food; life seems suspended, yet the head and mind remain clear and intelligent; there is a total absence of pain; neither does the patient complain of any symptom of local or general irritation; the heat of the skin is natural, and the pulse, in the midst of this dead suspense, continues its course steadily and quietly. This state of things lasts from twelve to twenty-four hours, when it ceases; and every organ is gradually restored to its former elasticity.

"But it should be borne in mind that such instances of great depression, produced by the acid, are extremely rare, and in-

deed seldom occur where tonics, or a proper quantity of nourishment can be given to the patient, at the time he is taking the acid. There is scarcely a remedy which does not, more or less, present some anomalies in its effects, dependant on the particular idiosyncrasy of the patient. In some few cases the prussic acid disagrees with the stomach, and then it neither can, nor ought to be, persisted in: it also occasions giddiness in some individuals; in which case it is necessary either to discontinue it, or diminish the dose, and associate it with slight stimulants. Opium, henbane, &c. have often, in this respect, exhibited the same occasional deviations from their usual mode of action.

"But the prussic acid has never yet been found to produce the head-ache and heaviness occasioned by laudanum; the fluttering and palpitations brought on by hemlock; nor the parched mouth and irritability of the throat, arising from the action of belladonna. The prussic acid, moreover, acts gently on the bowels, in the first instance, and when after some days they seem to fall into a torpid state, the mildest medicines, and those in smaller quantities than usual, suffice to produce the desired effect, when combined with the acid."

The author next proceeds to enumerate the diseases in which it has proved beneficial. Its effects "in almost every kind of cough, particularly of a spasmodic nature, are highly satisfactory." "In hectic fever it affords ease; lowers the pulse; diminishes the number of paroxysms; works a favourable change in the action of the lungs and their circulation, while the morbid heat of the skin, and the circular flushes of the cheeks, gradually disappear. The night sweats are also soon suspended." p. 21. "In the treatment of confirmed consumption, the prussic acid, even at the approach of death, is the most advantageous palliative that can be employed. Greatly superior, indeed, to any hitherto adopted." p. 22. Asthmatic complaints are also stated to have been greatly relieved by it; and from analogy, the author is sanguine enough to expect much benefit from it in spasms of the stomach and diaphragm, and even in "locked jaw, tic douloureux, and, perhaps, hydrophobia!" p. 23. "Credat Judæus appella." The following

instance, however, we can fully credit: "but I have had no experience of its utility in such affections."

Dr. G. has successfully employed this medicine in the dry convulsive spasmodic cough, which may be called sympathetic, as depending entirely on a morbid state of the liver, stomach, spleen, &c. With regard to hooping cough, "it may be stated, without presumption, that no case need be suffered to proceed longer than eight or ten days if the prussic acid be timely and cautiously administered; and, it is singular, that children bear the action of this sedative medicine, in small doses, better than adults." p. 24. In inflammatory affections of the lungs, which have a tendency to recur on the slightest cause, and to terminate in hectic and purulent expectoration, and after depletion cannot be carried any further, Dr. G. thinks the prussic acid will produce the most advantageous effects; and also in those pains which attend and follow abortions, and in hæmoptysis. p. 25, 6, 7. Some cases follow, illustrative of its good effects in consumptive and other cases above mentioned, by the author, Dr. Majendie, Mangini, Scudamore, and Mr. Thompson, of Sloane street; and, according to their observations, no bad effect had ever resulted from this medicine, though great care is admitted to be requisite in its exhibition.

Towards the conclusion of the volume is a valuable communication from the latter gentleman, with a detail of four cases, in which he exhibited this medicine. He observes, "I have prescribed the acid in a great number of instances, with variable success; but the benefit it has produced is amply sufficient to authorise me to add my testimony in favour of its value as an important addition to the host of direct sedatives." p. 67.

The 8th section is occupied with the mode of prescribing the prussic, or hydrocyanic acid. It decomposes most of the salts used in medicine, particularly those of antimony and soda. Its affinity for the alkalies and earths is inconsiderable; that for the former is so weak, that even carbonic acid displaces it. Hence it may be given with carbonate of potash, forming one of the most successful modes of prescribing it in spasmodic and hooping coughs. It cannot be administered with the sulphurets. It may be given in vegetable infusions,

with the addition of syrups if necessary. With tonics it may be associated with real advantage, as with the filtered infusions of bark, columbo, cascarilla, or even sarsaparilla, or in incipient pulmonary complaints in a strong decoction of lichen islandicus. It is indispensably necessary to use no other than distilled water in all prescriptions with the prussic acid; otherwise decomposition will take place. p. 60, 63. Some formulæ follow, in which eight or ten minims of the acid are exhibited in six or eight ounces of the vehicle; a table-spoon full every two or three hours. p. 63. When the prussic acid produces nausea, vomiting, or dizziness, which it will do in some individuals on the very first or second day, it is adviseable to abandon it immediately, for there is no chance of its ever agreeing with the patient; but such effects have not occurred above five or six times in one hundred cases in which it has been exhibited. p. 39. During the first days it proves gently aperient; and when it has this effect, the mildest purgatives will suffice to produce the necessary evacuation. Mr. Thompson states his general practice in catarrhal affections and chronic coughs to have been, after purging, to give two minims of the acid in a spoonful of distilled water or almond emulsion, every two or three hours during the day, increasing the quantity two or three minims on the whole portion taken in the twenty-four hours, every day, until the cough was subdued. p. 70. The greatest amount to which it has been carried by him was twenty-four minims in the day to an adult, and six to an infant. For infants between four months and a year old, he prescribed two minims in ʒix of distilled water, with ʒj of the syrup of Tolu, coch. min. ji. 3tiis. horis sumenda. As to its modus operandi, he observes; "the prussic acid, when taken into the stomach, produces its action on the circulating system, evidently through the medium of the nerves, the energy of which it considerably lessens and even altogether destroys when the dose is sufficiently strong. In no case have I remarked that any excitement precedes its sedative effect, a circumstance which distinguishes it from every other substance belonging to the class of narcotics." p. 67.

There are some typographical mistakes which ought to have been noticed in a table of errata, as in the formula, page 76,

and at page 81, *ob*noxious is used instead of noxious. In presenting a short but faithful analysis of this little volume, we will not say, "cras credemus, hodie nihil," but we shall feel much contented, and deem the prussic acid no contemptible acquisition, if experience shall prove it to deserve even a moderate competency of the virtues and efficacy here ascribed to to it. We recommend this little work to the perusal of our brethren.

Reports on the Epidemic Cholera which has raged throughout Hindostan and the Peninsula of India, since August, 1817. *Published under the Authority of Government.* One Vol. 4to. 228 Pages. Bombay, 1819.

[From the Medico-Chirurgical Journal, for April, 1820.]

——————— seu dira per omnes
Manarent populos sævi contagia morbi.

THIS important series of documents, drawn up by the Medical Board of Bombay, has been presented to us, through the medium of Dr. Scott, by the desire of the head of that board, lately returned to Europe.* The work is circulating widely in India, but cannot, of course, be known here, except through such vehicles as the present. We deem it a duty, therefore, to the profession at large, to make them more intimately acquainted than they have hitherto been, with one of the most awful and fatal epidemics that ever ravaged our widely extended Indian empire. The event itself is extremely interesting to the profession in general, in a pathological and therapeutical point of view, independently of those numerous ties and associations by which we are linked to the fate of our Asiatic possessions. On all these accounts we shall be pardoned for the length to which our analysis may extend, especially as we shall strain every nerve to make it as concentrated as literary labour and typographical closeness can render it.

There are some curious particulars attending the history of

* Dr. Steuart, since deceased.

this epidemic, which are worthy of record. It first appeared in August, 1817, in Zilla Jessore, about 100 miles North East of Calcutta, but without any previous peculiarity of weather; being considered by the authorities on the spot, as of a local nature, and attributable to the intemperate use of rank fish and bad rice; but it rapidly spread through the adjoining villages, running from district to district, until it had brought the whole province of Bengal under its influence. It next extended to Behar; and, having visited the principal cities west and east of the Ganges, reached the upper provinces. Through the large cities here it made a regular progress; but it was otherwise in the more thinly peopled portions of country. "The disease would sometimes take a complete circle round a village, and leaving it untouched, pass on as if it were wholly to depart from the district. Then, after a lapse of weeks, or even months, it would suddenly return; and scarcely re-appearing in the parts which had already undergone its ravages, would nearly depopulate the spot that had so lately congratulated itself on its escape. Sometimes, after running a long course on one side of the Ganges, it would, as if arrested by some unknown agent, at once stop; and taking a rapid sweep across the river, lay all waste on the opposite bank." *Report of the Calcutta Medical Board.*

In Calcutta it shewed itself in the first week of September, and each succeeding week added strength to the malady, and more extended influence to its operation. From January till the end of May it was at its acmé; during which period, the mortality in the city was seldom under two hundred a week!

The centre division of the army, under the commander in chief, exhibited an awful specimen of the fatality of the disease. It consisted of less than ten thousand fighting men, and the deaths, within twelve days, amounted, at the very lowest estimate, to three thousand; according to others, to five, and even eight thousand!

On the 6th of August, 1818, it reached Bombay, taking about a year to cross the base of the Great Indian Delta. It appeared to Drs. Steuart and Phillips, the enlightened members of the medical board at Bombay, that the disease was capable of being "transported from place to place as in cases of

ordinary contagion or infection, and also to possess the power of propagating itself by the same means that acknowledged contagions do." *Preface*, xii.

The partial and irregular manner in which the disease spread and operated in the neighbourhood of Bombay, as the cold season advanced, could not be accounted for by the medical board, "unless by supposing that a diminution of temperature, together with exposure, may have called into action some latent remains of an active poison." The board next proceeds to a description of the disease, as drawn up by the medical board of Bengal, which we shall here introduce verbatim.

"Having thus given a rapid and imperfect sketch of the history of the epidemic, the board should now proceed to detail the symptoms which attended its attack. This part of their task they will not find it difficult to accomplish. The leading appearances of this most fatal malady were but too well marked on their approach and subsequent progress; and, amongst the myriads who were attacked, exhibited perhaps less variety and fewer discrepancies than characterize the operation of almost any other disease to which the human body is subject. The healthy and unhealthy; the strong and feeble; Europeans and natives; the Mussulman and Hindoo; the old and young of both sexes, and of every temperament and condition, were alike within its influence.

"The attack was generally ushered in by a sense of weakness, trembling, giddiness, nausea, violent retching, vomiting and purging, of a watery, starchy, whey-coloured, or greenish fluid. These symptoms were accompanied, or quickly followed by severe cramps, generally beginning in the fingers and toes, and thence extending to the wrists and fore-arms, calves of the legs, thighs, abdomen, and lower part of the thorax. These were soon succeeded by pain, constriction, and oppression of stomach and pericardium; great sense of internal heat; inordinate thirst, and incessant calls for cold water; which was no sooner swallowed than rejected, together with a quantity of phlegm, or whitish fluid, like seethings of oatmeal. The action of the heart and arteries now nearly ceased; the pulse either became altogether imperceptible at the wrists and tem-

ples, or so weak as to give to the finger only an indistinct feeling of fluttering. The respiration was laborious and hurried, sometimes with long and frequently broken inspirations. The skin grew cold, clammy, covered with large drops of sweat; dank and disagreeable to the feel, and discoloured, of a bluish, purple, or livid hue. There was great and sudden prostration of strength, anguish, and agitation. The countenance became collapsed; the eyes suffused, fixed and glassy; or heavy, and dull; sunk in their sockets, and surrounded by dark circles; the cheeks and lips livid and bloodless; and the whole surface of the body nearly devoid of feeling. In feeble habits, where the attack was exceedingly violent, and unresisted by medicine, the scene was soon closed. The circulation and animal heat never returned; the vomiting and purging continued, with thirst and restlessness; the patient became delirious or insensible, with his eyes fixed in a vacant stare, and sunk down in the bed; the spasms increased, generally within four or five hours.

"The disease, sometimes at once, and as if it were momentarily, seized persons in perfect health; at other times, those who had been debilitated by previous bodily ailment; and individuals in the latter predicament, generally sunk under the attack. Sometimes, the stomach and bowels were disordered for some days before the attack, which would then, in a moment, come on in full force, and speedily reduce the patients to extremities.

"Such was the general appearance of the disease where it cut off the patient in its earlier stages. The primary symptoms, however, in many cases, admitted of considerable variety. Sometimes the sickness and looseness were preceded by spasms; sometimes the patient sunk at once, after passing off a small quantity of colourless fluid, by vomiting and stool. The matter vomited in the early stages was, in most cases, colourless or milky; sometimes it was green. In like manner, the dejections were usually watery and muddy; sometimes red and bloody; and in a few cases, they consisted of a greenish pulp, like half digested vegetables. In no instance was feculent matter passed in the commencement of the disease. The cramps usually began in the extremities, and thence gradually

crept to the trunk; sometimes they were simultaneous in both; and sometimes the order of succession was reversed; the abdomen being first affected, and then the hands and feet. These spasms hardly amounted to general convulsion. They seemed rather affections of individual muscles, and of particular sets of fibres of those muscles, causing thrilling and quivering in the affected parts, like the flesh of crimped salmon, and firmly stiffening and contorting the toes and fingers. The patient always complained of pain across the belly, which was generally painful to the touch, and sometimes hard and drawn back towards the spine. The burning sensation in the stomach and bowels was always present, and at times extended along the cardia and œsophagus to the throat. The powers of voluntary motion were, in every instance, impaired, and the mind obscured. The patient staggered like a drunken man, or fell down like a helpless child. Head-ach over one or both eyes sometimes, but rarely occurred. The pulse, when to be felt, was generally regular, and extremely feeble, sometimes soft; not very quick; usually ranging from 80 to 100. In a few instances, it rose to 140 or 150, shortly before death. Then it was indistinct, small, feeble, and irregular. Sometimes very rapid, then slow for one or two beats. The mouth was hot and dry; the tongue parched, and deeply furred, white, yellow, red, or brown. The urine at first generally limpid, and freely passed; sometimes scanty, with such difficulty as almost to amount to strangury; and sometimes hardly secreted in any quantity, as if the kidnies had ceased to perform their office. In a few cases, the hands were tremulous; in others, the patient declared himself free from pain and uneasiness, when want of pulse, cold skin, and anxiety of features, portended speedy death. The cramp was invariably increased upon moving.

"Where the strength of the patient's constitution, or of the curative means administered, were, although inadequate wholly to subdue the disease, sufficient to resist the violence of its onset, nature made various efforts to rally; and held out strong, but fallacious promises of returning health. In such cases, the heat was sometimes wholly, at others partially restored; the chest and abdomen in the latter case becoming

warm, whilst the limbs kept deadly cold. The pulse would return; grow moderate and full; the vomiting and cramps disappear; the nausea diminish, and the stools become green, pitchy, and even feculent; and with all these favourable appearances, the patient would suddenly relapse; chills, hiccup, want of sleep, and anxiety, would arise; the vomiting, oppression, and insensibility, return; and in a few hours terminate in death.

"When the disorder ran its full course, the following appearances presented themselves. What may be termed the cold stage, or the state of collapse, usually lasted from twenty-four to forty-eight hours, and was seldom of more than three complete days' duration. Throughout the first twenty-four hours, nearly all the symptoms of deadly oppression, the cold skin, feeble pulse, vomiting and purging, cramps, thirst and anguish continued undiminished. When the system shewed symptoms of revival, the vital powers began to rally; the circulation and heat to be restored; and the spasms and sickness to be considerably diminished. The warmth gradually returned; the pulse rose in strength and fullness, and then became sharp and sometimes hard. The tongue grew more deeply furred; the thirst continued, with less nausea. The stools were no longer like water; they became first brown and watery; then dark, black, and pitchy; and the bowels, during many days, continued to discharge immense loads of vitiated bile, until, with returning health, the secretions of the liver and other viscera gradually put on a natural appearance. The fever, which invariably attended this second stage of the disease, may be considered to have been rather the result of nature's effort to recover herself from the rude shock which she had sustained, than as forming any integrant and necessary part of the disorder itself. It partook much of the nature of the common bilious attacks prevalent in these latitudes. There was the hot dry skin; foul, deeply furred, dry tongue; parched mouth; sick stomach; depraved secretions, and quick variable pulse; sometimes with stupor, delirium, and other marked affections of the brain. When the disorder proved fatal after reaching this stage, the tongue, from being cream-coloured, grew brown, and sometimes dark, hard, and more deeply

furred; the teeth and lips were covered with sordes; the state of the skin varied; chills, alternating with flushes of heat; the pulse became weak and tremulous; catching of the breath; great restlessness, and deep moaning succeeded; and the patient soon sunk, insensible, under the debilitating effects of frequent dark, pitchy, alvine discharges.

"Of those who died, it was believed, perhaps rather fancifully, that the bodies sooner underwent putrefaction, than those of persons dying under the ordinary circumstances of mortality. The bodies of those who had sunk in the earlier stages of the malady, exhibited hardly any unhealthy appearance. Even in them, however, it was observed, that the intestines were paler, and more distended with air, than usual; and that the abdomen, upon being laid open, emitted a peculiar offensive odour, wholly different from the usual smell of dead subjects. In the bodies of those who had lived some time after the commencement of the attack, the stomach was generally of natural appearance, externally. The colour of the intestines varied from deep rose to a dark hue, according as the increased vascular action had been arterial or venous. The stomach, on being cut into, was found filled, sometimes with a transparent, a green, or dark flaky fluid. On removing this, its internal coats, in some cases, were perfectly healthy; in others, and more generally, they were crossed by streaks of a deep red, interspersed with spots of inflammation, made up of tissues of enlarged vessels. This appearance was frequently continued to the duodenum. In a very few cases, the whole internal surface of the stomach was covered with coagulable lymph; on removing which, a bloody gelatine was found laid on the interior coat, in ridges or elevated streaks. The large intestine was sometimes filled with muddy fluid, sometimes livid, with dark bile, like tar; just as the individual had died in the earlier or later periods of the attack. In most cases, the liver was enlarged, and gorged with blood. In a few, it was large, soft, light-coloured, with greyish spots, and not very turgid. In others again, it was collapsed and flaccid. The gall-bladder was, without exception, full of dark green or black bile. The spleen and thoracic viscera were, in general, healthy. The great venous vessels were usually gorged;

and in one case, the left ventricle of the heart was extremely turgid. The brain was generally of natural appearance. In one or two instances, lymph was effused between its membranes, near the coronal suture, so as to cause extensive adhesions; in other cases, the sinuses, and the veins leading to them, were stuffed with very dark blood." xv.—xxi.

The following extracts will show that the disease was known to Sydenham, and accurately described by that observant physician. He no where mentions bile as forming any part of the discharges from the stomach or bowels; and hence, it may be fairly inferred, that such discharges were not present.*

"Qui ab ingluvie ac crapula nullo temporis discrimine passim excitatur affectus, ratione symptomatum non absimilis, nec eandem curationis methodum respuens, tamen alterius est subsellii. Malum ipsum facile cognoscitur, adsunt enim vomitus enormes, ac pravorum humorum cum maxima difficultate et angustia per alvum dejectio; cardialgia, sitis. Pulsus celer ac frequens, cum æstu et anxietate, non raro etiam parvus et inæqualis, insuper et nausea molestissima, sudor interdum diaphoreticus, crurum et brachiorum contractura, animi deliquium, partium extremarum frigiditas, cum aliis notæ symptomatibus, quæ adstantes magnopere perterrefaciunt, atque etiam angusto viginti quatuor horarum spatio ægrum interimant."

And again, in his letter to Dr. Brady, describing the epidemics of 1674, 5, and 6, he says,

"Exeunte æstate Cholera Morbus epidemice jam sæviebat,

* We have diligently searched the writings of Sydenham, and we assert, that in no one instance, when treating of cholera morbus, whether epidemic or sporadic, has he mentioned a discharge of *bile* as forming any part, much less as being the *cause* of cholera. And as Sydenham is allowed to be one of the most accurate observers of nature, we see on what foundation Dr. Saunders and others have built their *bilious* theory of the disease. The fact is, as we have long ago stated, that the discharge of bile in cholera, is a secondary or ternary link in the chain of cause and effect—and always a sanative effort of the system, as well as a favourable symptom of the disease.

We observe too, that Aretæus describes the discharge of *bile* as only an ulterior effect. "In primis," says he, "quæ evomuntur, *aquæ similia sunt;* quæ anus effundit, stercorea, liquida, tetrique odoris sentiuntur. Siquidem longa eruditas id malum excitavit, quo si per clysterem eluantur, *primo pituitosa, mox biliosa feruntur.*"—*De Cholera*, Chap. 5.

et insueto tempestatis calore evectus, atrociora convulsionum symptomata, eaque diuturniora secum trahebat, quam mihi prius unquam videre contigerat. Neque enim solum abdomen, uti alias in hoc malo, sed universi jam corporis musculi, brachiorum crurumque præ reliquis, spasmis tentabantur dirissimis, ita ut æger e lecto subinde exiliret, si forte extenso quaquaversum corpore eorum vim posset eludere." xxiii.

The first of the foregoing extracts describes the disease with great accuracy, as it very generally affected the natives; the second is well exemplified in Dr. Burrell's Report, as it attacked the Europeans of the 65th regiment, at Seroor. The disease is also accurately described by Girdlestone, and by Mr. Curtis of Madras, in 1782, when it raged in the southern provinces of the peninsula. Dr. Taylor also furnished the Medical Board with the account of a disease from a Sanscrit medical work, the MADHOW NIDAN, which clearly proves that the complaint has been long known to the natives.

"It is obviously unnecessary to prosecute this inquiry further; and we shall only add, that Dr. James Johnson is the latest author, so far as we know, who has treated this subject, and who has also the merit of having been the first who has generally pointed out the best method of cure, from a few cases he met with on the eastern coast of Ceylon, where the disease seems to be more prevalent than in any other part of India." xxviii.

The exciting and approximate causes of this interesting epidemic are, like those of most others, concealed in utter darkness—"*atra caligine mersæ;*" great discrepancy of opinion obtains in India respecting its contagious or non-contagious influence, arising naturally out of the difficulty of the subject.

"Several irresistible facts already noticed, or related in the following reports, and its marked anomaly from all hitherto known simple epidemics, would seem to favour the doctrine of contagion, while the contrary supposition is only supported by a species of negative evidence." xxix.

The board, however, very properly observe, that this is a question of such importance, that it ought not to be too hastily entertained as proved, nor rejected as unfounded; but

prosecuted with that diligent inquiry and cautious induction, which, on every subject of science, are so necessary to the attainment of truth.

In respect to the predisposing [or rather the *exciting*] causes, practitioners are unanimous.

"Rapid atmospherical vicissitudes, in regard either to temperature or moisture; exposure of the body to currents of cold air, particularly the chill of the evening, after being heated by violent exercise of any kind, inducing debility or exhaustion; low marshy situations; flatulent or indigestible food, especially crude and watery vegetables, which compose a large proportion of the diet of the natives; and particularly that gradual undermining of constitution which arises in a condensed, dirty, and ill-fed mass of population, are all unquestionably powerful predisposing causes."

Sad experience, however, has shown that the absence of all these afforded no security against the attack. Whether the invisible cause, whatever that may be, acts more immediately on the vascular or nervous system, the board cannot take upon them to determine; but from the various modes of attack which gave rise to the division of the disease into two species and varieties, they are led to the supposition that sometimes the one system, sometimes the other, bears the onus of the first onset of the malady.

"The most general attack seems to consist in a spasmodic affection of the stomach, duodenum, and more especially the biliary ducts; the total absence of bile in the matter voided upwards and downwards being, perhaps, the most uniform characteristic of the disease, which, extending through the whole intestinal canal, discharges its contents. It is more than probable, however, that these are merely the first perceptible symptoms; for it would appear that a great change has already taken place in the circulating system, and that the action of the heart itself has been greatly diminished before they occur. This seems evident from the numerous cases in which neither vomiting nor purging is present, and in which the first appearance of the disease is the almost total suspension of the vital functions, immediately followed by severe spasmodic

affections of the muscles and coldness of the extremities." xxxiii.

Here the board have copied Dr. Armstrong's description of the attack of *congestive* typhus, remarking that,

"Those who are most intimate with the disease in question, will be struck with the great similarity between this and typhus, at their first appearance."

Dissections, they state, abundantly prove that venous congestion constitutes the principal change that takes place during life.

The following passage, though long, cannot be abridged, without greatly lessening its value.

"On the subject of the cure of the disease, we need say but little. The practice so judiciously and speedily adopted by Dr. Burrell in the 65th regiment, clearly proves, that at the commencement of the disease in Europeans, blood-letting is the sheet anchor of successful practice; and perhaps also with natives, provided it be had recourse to sufficiently early in the disease; and as long as the vital powers remain, so as to be able to produce a full stream, it ought perhaps never to be neglected; it having been sufficiently proved, that the great debility so much complained of is merely apparent. Calomel, as a remedy, certainly comes next in order, and when employed in proper doses, with the assistance of opium, and more particularly in the early stage of the disease, seems to be equally effectual among natives, as venesection among Europeans, in arresting its progress. In all the cases formerly alluded to, when we met the disease on its first attack, a single scruple dose of calomel, with sixty minims of laudanum, and an ounce of castor oil seven or eight hours afterwards, was sufficient to complete the cure. The practice of this place, as sufficiently appears by Dr. Taylor's report, bears ample testimony to the control which calomel possesses over the disease, in as much as it has often preserved life, when blood-letting could not be put in practice.

"All other remedies must, in our opinion, be considered as mere auxiliaries, no doubt extremely useful as such, and ought never to be neglected; but particularly the warm bath and stimulating frictions. Even where the disease appears to

have given way to bleeding, we think it highly necessary constantly to administer calomel. The powerful effect of this remedy in allaying irritability of the stomach and intestines, when given in large doses, is generally acknowledged by practitioners, in the severer attacks of dysentery: as a great and permanent stimulus to the vascular system, it will be readily acknowledged by every one who has suffered for any length of time under its effect in ptyalism, where the bounding pulsations of the arteries of the temples and neck produce very disagreeable sensations, and even preclude sleep. Its powers over inflammation of the abdominal viscera, the liver in particular, and indeed, in membranous and glandular inflammation generally, are now universally acknowledged.*

* "We read with some surprise the declared opinion of Dr. Armstrong, in his Treatise on Puerperal Fever, that the good effects of calomel were solely owing to its purgative quality; while, at the same time, he acknowledges that the disease was both more speedily and perfectly overcome in those cases where ptyalism was produced. He has, however, made ample amends in the treatise now before us, where he acknowledges that its value is to be attributed to its specific qualities as a mercurial. It is by the acknowledged errors of such men that medical practice is stored with its most valuable facts. Puerperal fever is not a very common occurrence in this country [India], although it occasionally takes place; and it is but an act of justice due to Dr. Helenus Scott, formerly of this place, now of Russel Square, to state, that for more than thirty years back, he was in the constant practice of treating this fever with calomel so as to affect the system; and that, to the best of our recollection he never lost a patient. We are led to make this observation from another motive, as we have reason to believe, that this salutary practice is but little known in this country, and it may serve as a hint which may save some valuable lives."

R. STEUART,
B. PHILLIPS.

The following Communication from Mr. John Scatchard, *of East Keswick, near Leeds, will come in opportunely here.*

"In the treatment of puerperal fever, by general professional consent, large doses of the submuriate of mercury are allowed to be of very great utility, especially when exhibited opportunely, and in concert with other well known auxiliary means. Dr. Armstrong in his valuable work on the above disease has very successfully employed this remedy, and thus recommends its more extensive adoption. It appears, however, from what we may collect from his work, p. 65, 1st. edit. and p. 10, 88, 94, 2d edit., that neither the author, nor any of his intelligent correspondents, had recourse to this medicine, in such quantity at least, previously to January, 1813.

"Now, if your readers will take the trouble to refer to the 145th and 157th numbers of the Medical and Physical Journal, particularly the former, it will be seen, that Dr. Bradley of Huddersfield, administered large doses of calomel in puerperal fever, with the most unequivocal success, at least two years before Dr. Armstrong adopted the same remedy. Moreover, a reference to these cases will demonstrate,

"In a disease, therefore, in which we have every reason to believe that venous congestion has taken place to a great extent, and where we conclude that the liver, from its peculiar circulation and structure, is more immediately liable to become seriously and permanently injured, it should not be omitted. We have before mentioned, that Dr. James Johnson seems to have been the first who pointed out the best method of cure. Since most of the foregoing remarks were written, we have seen the second edition of that gentleman's valuable work, in which we find a strong corroborative testimony to the utility of blood-letting in this disease, or one somewhat similar to it, on the coast of Brazil, by Mr. Sheppard of Witney, without the assistance of any other remedy. The public are greatly indebted to Mr. Corbyn, of the Bengal establishment, for his clear and comprehensive letter on this subject, at a time when the disease was producing the most dreadful ravages: the early communication of his practice has been the means of saving thousands of lives in situations where Dr. Johnson's work might not be known." xlii.

About forty official reports, from various medical officers, compose the great body of the work before us, and form the materials from which Drs. Steuart and Phillips have drawn up the foregoing luminous and interesting digest. It is not ne-

that in Dr. Bradley's hands, a combination of calomel, with opium, was beneficially employed in one instance; and in the second edition of his work, Dr. Armstrong has fully canvassed this remedy. Whether the striking coincidence herein evinced were fortuitous or otherwise, it is impossible to pronounce with certainty.

"The priority of adoption and publication of the above mode of exhibiting calomel, for the cure of puerperal fever, I believe, unquestionably belongs to Dr. Bradley.

"The two papers above referred to, contain the essence of almost every thing valuable that has been written on the subject of puerperal fever, since Dr. Gordon's inestimable publication on the same disease. In those papers, blood-letting and copious purging are inculcated; and more definite criteria, respecting the indications and proper period for having recourse to the former remedy, are there pointed out, than are elsewhere to be met with. And what, I would ask, has the science of medicine, as yet, added to these means of cure, strictly speaking?

"Should Dr. Armstrong peruse these remarks, he will pardon the freedom with which they are made, and be assured that no one can have a more profound respect for his great talents than I entertain; yet, since every man is entitled to the benefit or credit of his own discoveries, I have deemed it but right unreservedly to assert the prior claim of its legitimate author to the above improved practice."

JOHN SCATCHARD.

cessary to go into these reports individually. There never perhaps existed so unanimous a consent respecting the treatment of such a wide-spreading epidemic, as these documents disclose. The pre-eminent powers of blood-letting and mercury in diseases of the eastern world, are now so firmly established on the basis of facts, that it would be ingloriously to tread over a prostrate enemy, to even notice the dreams of the Brunonians, and the scepticism of the anti-mercurialists.

Medical Notes on Climate, Diseases, Hospitals, and Medical Schools, in France, Italy, and Switzerland; comprising an Inquiry into the Effects of a Residence in the South of Europe, in Cases of Pulmonary Consumption, and illustrating the present State of Medicine in those Countries. By JAMES CLARK, M. D. London, 1820. pp. 250. Octavo.

[From the Medico-Chirurgical Journal, for April, 1820.]

WE were lately so completely *taken in* by a goodly volume bearing a title something similar to the one now before us, that it was not without some hesitation that we ventured upon the purchase and perusal of Dr. Clark's. We were indeed, somewhat re-assured by the modesty of the title page, and the unpretending humbleness of the preface; but we have waxed too wise and sober on our critical Pharos to risk much of our judgment on such appearances—fronti nulla fides—and it was only after having clandestinely run over, in our bookseller's, some of the pages in the *interior*, that we ventured to order its translation to our own shelves. In this instance, we are happy to say that we have not been disappointed. Dr. Clark gives us more than he promises but certainly much less than we could wish. Indeed, on many occasions his remarks are so brief and few, that even the title of the volume (Notes) and the apologetical circumstances mentioned in the preface, were insufficient to satisfy our craving for further information, which was only irritated by the little that was afforded us. Upon the whole, however, we have been much pleased and

instructed by this little work of Dr. Clark, and we can very conscientiously recommend it to our readers, as containing much useful and interesting matter.

It appears by the preface, which is in the form of a letter, dated Rome, Nov. 1819, and addressed to Dr. Forbes of Penzance, that the Notes now offered to the public were taken under very varying circumstances, over which the author had frequently no control, of time and convenience, during casual visits to the places noticed; and that they are now presented to the public chiefly at the instance of the author's friends, from a conviction that they contain matter both valuable and interesting, and from apprehension lest events may prevent the accomplishment of the hope he entertains of being hereafter enabled to fill up the outline he has sketched.

Dr. Clark's book is divided into two parts: the first comprehending his observations on the climate and endemic diseases of some parts of the south of France, Italy and Switzerland; the second his account of the state of medicine in some of the hospitals and medical schools of the same countries. In collecting the materials for the first part of his work, the object of Dr. Clark has evidently been to endeavour to ascertain the effect of the climates of the south of France and Italy on those cases of consumption sent thither annually, in such numbers from England; to determine whether this practice is at all useful or injurious; in what cases and stages of the disease it is so; and which, of all the various places resorted to, is the most eligible. In the fulfilment of this plan, in addition to the examination of the topography and local habitudes of the different places, he has naturally been led to inquire into the nature and degree of prevalence of the diseases met with in them, especially of such as have any relation to consumption, or to those qualities of climate found to influence its developement or progress. In this inquiry we are frequently presented with very interesting notices of diseases not particularly connected with the main subject of investigation; also with curious dietetic notices, and valuable practical remarks. We are, also, presented with numerous highly useful directions for the guidance of the invalids in the different places frequented by them, and many cautions and warnings, highly

important to this class of sufferers; and which, by the earnestness with which they are endeavoured to be impressed, very advantageously illustrate the feeling and kind-heartedness of the excellent author. In reference, particularly to this part of the book, we consider it as constituting a most excellent guide for the invalid who visits these countries, and earnestly recommend it to all such, and to professional men who may accompany them, as fraught with much that it imports them to know, and which they cannot otherwise acquire than by much and hazardous experiment.

Dr. Clark's experience has led him to form a very low estimate of the beneficial influence of the climate of the south of France and Italy in pulmonary consumption. Indeed, the impression left on our minds by an attentive perusal of his work, of the inutility, nay, of the impropriety of sending consumptive patients thither from this country, is very strong, much stronger than that which seems to have been left on the mind of the author himself; certainly infinitely stronger than our previous imperfect information on this matter had led us to entertain. The author sums up his opinion on this subject in the following words, in which we are, in the main, disposed to agree with him.

"To sum up in a few words the opinion I have formed from all the observations I have been enabled to make on the effects of climate in pulmonary consumption, it appears to me, then, that the change of our English climate for a residence in the milder ones of the south of Europe, is much more beneficial as a preventive of the disease, than, I fear, it will ever be found as a means of cure of it when formed. In the young and growing members of delicate, scrofulous and consumptive families, however, continued for some winters during that age when the body is attaining its full growth, and when catarrhal affections are attended with the greatest danger, it may have great influence in checking the tendency to hereditary disease. Even when tubercles already exist in the lungs in a state of irritation, a residence for some years in a mild temperature, together with the adoption of a proper regimen, may be the means of allaying the irritation, and consequently of preventing the suppuration of these tubercles. By a little fu-

ture attention in guarding against the known exciting causes of inflammation, these may long, and perhaps for life, remain in a state of quiescence. By such measures, and a strict adherence to the other means most proper for strengthening the constitution, and by acquiring habits calculated to inure the body to the cold and inequalities of its native climate, among which I consider the habitual use of the cold bath as pre-eminent, I have no doubt that many lives might be saved. When, however, suppuration has actually taken place in the substance of the tubercles, my opinion is, that little or no benefit is to be expected from a change of climate in the cure of the disease; and further, that by the great and numerous inconveniencies and discomforts of so long a journey, the fatal termination of it is more frequently accelerated than protracted. That this is very frequently the case in the very advanced stages of the disease, such as I have frequently met with on the continent shortly after their arrival from England, I have no manner of doubt." p. 114.

The places whose fitness for winter residence for the consumptive are investigated by Dr. Clark, are Marseilles, Hieres, Nice, Villa Franca, Pisa and Rome; and the plan he has followed in his examination of these appears to us very judicious. It is thus briefly stated by himself, and he seems to have faithfully adhered to it.

" I shall give in the ensuing remarks, first, a short topographical account of each, confining myself, however, strictly to those circumstances which appear to me immediately connected with its locality in a medical point of view; secondly, observations on its climate; and, thirdly, remarks on the diseases in which it seems useful or injurious, founded on these observations, and on a knowledge of the ailments to which the inhabitants are most liable. To these will be occasionally added a few remarks on the present state of medical practice, as far as sufficient information could be gained on that point." p. 7.

Of all the places described, Marseilles seems to be, on every account, the most improper as a residence; and, indeed, the objections to it detailed by Dr. Clark are so great and obvious, that it would be truly most surprising that it should still continue to be resorted to by any of our countrymen,

were it not for the very remarkable ignorance of the profession in this country, of the nature of the climate and local habitudes of the whole south of France. This ignorance we do not say that Dr. Clark's little work will entirely remove; indeed, it is much too brief to have this effect; but it is certainly more calculated to do so than any other work we have yet met with.

Marseilles is very much exposed to the violent, cold, piercing N. W. winds, termed by the natives *mistral;* which, indeed, are felt throughout the whole northern shores of the Mediterranean with remarkable severity.

"The great evil of Marseilles, as a winter residence to the consumptive, or those liable to inflammatory affections of the chest, is the frequency and force of the dry cold northerly winds; to which, during that season, it is particularly subject, and to the full influence of which its northerly aspect exposes it in a peculiar manner. The effect of these winds, the most frequent and violent of which is the north-west, termed by the inhabitants the *mistral,* is, to produce a very great, and often very sudden alteration of temperature. This is severely felt by the inhabitants themselves, the surface being probably relaxed, and rendered particularly sensible, by the preceding heats, and on many occasions cannot be guarded against by the most cautious invalid. The influence of this wind in sinking the thermometer is by no means in proportion to its effects on the living body."

This unfavourable exposure, and cold variable climate, appears to be extremely productive of disease of the pulmonary organs. Consumption is more prevalent in Marseilles than in most of the towns of France; its progress is stated to be extremely rapid; and is considered by the medical men as invariably fatal.

Females from 14 to 18 years of age are stated to be its most frequent victims. Its ravages among the youth are stated in very energetic terms by Dr. Segand, in a memoir quoted by our author. "Elle fait des ravages inouis en moissonnant la plus belle jeunesse!" p. 14.

Another evil of this place, considered as a residence for invalids, is said to be the local configuration of its vicinity, which

is extremely confined and hemmed in by the neighbouring mountains, and by the thick high enclosures of the fields and gardens. p. 10.

Hieres, a place of much higher repute as a station for the consumptive, is admitted by our author as justly entitled to this comparative praise; but he is by no means disposed to fall in with the high opinion entertained by many of its actually great advantages in this disease. "Its exposure is better, and it is in some degree protected by its finely clothed hills from the northerly winds, which, if not warded off, are at least broken in their violence." p. 25. This is comparatively good report, but even this is weakened by a remark in the preceding page.

"It is not well sheltered from the north-east, and still less so from the north-west, which last we have noticed as blowing during the greater part of December, so as to render it unsafe for a consumptive patient to venture out of doors, except in some corner, where he is exposed to the sun's rays, and completely sheltered from it." p. 24.

Like most of the towns on the southern shores of France, Hieres is surrounded on the land side by hills, and as these lie to the north of it, they tend to shelter it much more than Marseilles. The country around it is described as being extremely beautiful and fertile, but it seems the state of the cultivation, and inconveniencies of travelling, as at Marseilles, prevent invalids enjoying the advantages that the natural configuration of the place seems to afford in some situations.

"It is true, that about the bases of the hills there are some spots sheltered from the blasts of the mistral, where the invalid might enjoy several hours in the open air almost every dry day, but the difficulty is to reach them at the time that they would be most useful. The chilly blast, sweeping round every exposed corner, forbids the valetudinarian venturing there, except in a close carriage, while, on the other hand, the roads leading to these places do not admit wheeled carriages." p. 21.

Consumption is said to be much less frequent than at Marseilles.

The next place described by Dr. Clark, is *Nice*, which is

also situated close on the shore, and is bounded on the whole of the land sides by an amphitheatre of lofty mountains. The topographical advantages of this place, considered as a residence for the consumptive, seem to be very considerable, and much superior to any of the stations in the south of France. From the arrangement of its mountains, above noticed,

"Nice derives the superior mildness of its climate. While, open to the genial influence of the south, its mountain barrier on all other sides defends it in a great measure from the northerly winds, and particularly from the north-west or mistral. It is not altogether so well sheltered from the north, or rather north-east wind, which, sweeping down the valley of the Paglion, is not unfrequently felt during the winter and spring months with considerable severity, but in a degree much inferior, both in frequency and force, to that of the mistral in Provence." p. 29.

This superiority of the climate of Nice is shewn in the luxuriance of its vegetable productions, and the extreme fertility of its soil. As a *winter* climate, indeed, Nice seems to possess every quality of equability and mildness, which are usually esteemed the most essential in the disease to which these observations refer. We fear, however, it will be found, that something more is wanting to constitute Nice, or any other place, an advantageous residence for the consumptive, than the degree of mildness and equability which it seems to possess. At least from our author's account of its diseases, we are led to this conclusion. As a *spring* climate, it seems to fail much more in these requisites.

"Nice is protected from the dreaded mistral, but is still subject to some winds, chiefly the north-east, east, and south-east, which, though they do not blow with the violence of those of Provence, nevertheless are sharp and cold. They are particularly frequent during the spring months, and form a strong objection, in my opinion, to the climate, as a spring residence for the consumptive." p. 36.

Consumption is stated to be a very frequent and fatal disease at Nice; and catarrhal affections are mentioned by the medical men of the place as being their most prevalent complaints. An extract is given from an unpublished work of the

celebrated professor Foderé of Strasbourg, who spent upwards of six years at Nice; from which it appears, that this physician considers Nice as *particularly hostile* to the pulmonary organs. He considers it as beyond doubt, not only that consumption is more prevalent at Nice, and, indeed along the whole of these shores, than in most other situations, but that it is much more rapid in its progress, and fatal in its tendency.

"It seems irrefragably proved, by the experience of our time, that the climate of the shores of the Mediterranean is hurtful to such invalids. I had seen a great number of these cases terminate fatally at Marseilles; and, at that time I considered the very dry and keen winds of that place, as the principal source of evil; but I afterwards found that the warmer, softer, and more humid atmosphere of Nice, was in no respect more favourable to such complaints. Every case of hereditary tubercular phthisis proves fatal, as well at Nice as at Villa Franca, in very early age. In these places consumption is not, as in Switzerland, on the banks of the Soane, and in Alsace, a chronic disease: on the contrary, I have very often seen it terminate in forty days. A physician of the countries just mentioned, would be surprised at the quickness with which one attack of hæmoptysis succeeds another, how readily the tubercles suppurate, and how speedily the lungs are destroyed. The English annually make fresh experience of this melancholy fact, and their burying ground in the *Croix de Marbre* too well testifies its truth." 48, 49.

Another document, quoted by Dr. Clark, of great value and interest, as illustrating and confirming his opinions of the bad effects of the climate of the northern shores of the Mediterranean, is a Thesis lately published at Edinburgh, by Dr. Sinclair, a navy surgeon, who had served several years in our fleet, in the Mediterranean, and particularly on the coasts of France. This gentleman states consumption, or at least inflammatory affections of the lungs, terminating in suppuration, and ending fatally, to be much more prevalent among our seamen on that station than elsewhere; a difference which he attributes to the extreme variability of the climate. It appears from official documents, quoted by Dr. Sinclair, that,

"During the years 1810, 1811 and 1812, there were admitted into the naval hospitals of the Mediterranean, from the fleet, which contained about thirty thousand men, four hundred and fifty-five cases of phthisis pulmonalis, and one hundred and forty of pneumonia,—making a proportion of one in sixty-five affected with the former complaint, or of one in fifty, when the two diseases are taken together." 52.

Upon the whole, Dr. Clark seems to have left Nice with a very unfavourable impression of it as a residence for the consumptive.

Villa Franca, situated near Nice, only on the other side of Montalbano, is said to be still milder in point of temperature than Nice. It seems, however, to possess no essential advantages over it.

"This place, I am of opinion, must share the fate of Nice, as to its climate, whatever that may be; for though more sheltered from northerly winds, it has all the other good and bad qualities of the latter place, with the additional disadvantages of possessing few or no accommodations for invalids." 63.

Pisa is described as being less sheltered by mountain barriers than Nice, as having a more variable climate, and as being considerably colder; the only advantage it is stated to possess over this place is, "that there are good roads leading from it to all parts of Italy, and the invalid may leave it with safety much sooner than he could Nice."

This reminds us of the well known bon-mot of Johnson, who, when asked of the finest prospect he had seen in Scotland, mentions the *road to London!* It was, therefore, with some surprise, we found our author, in a subsequent part of his book, p. 113, giving Pisa a decided superiority over Nice as a winter's residence for the consumptive. He certainly does not acquaint us with the reasons for this preference. The last place described from personal observation, as a station for consumptive patients, is *Rome*. The following extract will give our readers some idea of the climatorial habitudes of this famous city, as a winter residence for the consumptive.

"The climate of Rome differs considerably from that of Nice and the southern parts of Provence. It is more moist.

The dry cold winds experienced at these places are comparatively little felt at Rome." 70.

"It is chiefly during the spring months, it appears to me, that Rome has the advantage in point of climate, being less liable to the keen cutting winds of the places I have already noticed. This is, however, an important circumstance, as the great difficulty is to find a good spring climate for the consumptive patient; the cold winds, which that season is liable to, I believe over the whole of Europe, being the evil most to be dreaded. At Naples they are particularly complained of. In truth, I believe no place is exempt from them; nor must I be understood as stating Rome to be so: all I wish to be understood is, that Rome is less liable to these winds, than most places I have seen." 71.

As corroborating the opinion the author had been led to form of the favourable nature of the climate of Rome, he quotes, from his own experience, several cases which appear to have been benefited by a residence there; but they are by far too few to permit us to rely much on the evidence they afford. Consumption appears to be a very prevalent disease at Rome; also inflammation of the chest and lungs. Dr. Clark does not appear to have visited *Naples*, but he has been led to consider it as much less favourable for consumptive patients than Rome. He compares it to Nice, in respect to its climate.

After these details respecting the comparative advantages and disadvantages of the various winter residences recommended for the consumptive, Dr. Clark proceeds to examine the question of a *summer residence*. He considers the whole of the south of France and Italy as improper residences during the summer months; and our experience of this disease in the warmer climates of the globe, strongly corroborates this opinion. The following extracts from this part of the work are interesting:

"I believe I shall not err, if I say that the whole of the south of France and Italy are improper for consumptive patients during the summer months, and that the more certainly, the farther the disease is advanced: where hectic fever is formed, and ulceration of the lungs has taken place, a high

temperature certainly accelerates the progress of the disease." 87.

"Though I do not think the observations of Dr. Sinclair are quite conclusive against the good effects of the climate of the Mediterranean and its shores on the generality of cases of pulmonary consumption which occur in England, and are sent abroad to pass the winter months only, they certainly show the impropriety of such patients remaining in that climate during the heats of summer, an observation which I have found corroborated by all the medical men of my acquaintance who have had opportunity of judging.

"It is rather a singular circumstance, that while the English practitioners were sending their consumptive patients to the shores of the Mediterranean, our naval medical officers navigating that sea were sending theirs to England.

"The same injurious effects in hurrying on the fatal termination of consumption, after ulceration had commenced, have been communicated to me by many of my naval friends who had served in the West Indies, as uniformly observed there. And I know that a practice, formerly prevalent in the navy, of sending seamen affected with this disease to that country, in the hope of attaining an alleviation of their complaints, has been of late years abandoned, owing to the representation of the naval medical officers of that country. From these facts, and also from my own observations in the countries themselves, I can have no doubt, nor indeed, do I believe, is there any doubt of the impropriety of consumptive patients remaining in the south of France and Italy during the summer months." 88.

This experience of the evil influence of the high summer heats of Italy is practically acted on by invalids, who are in the habit, it seems, of passing into Switzerland during this season, and returning to Italy in the winter. The places chiefly frequented by them are the neighbourhood of Lausanne and Geneva, which appear to possess no other claim to preference, than the circumstance of being cooler than Italy, and in lying conveniently in the route to that country. The invalids are recommended to reach Switzerland in the middle of June, and to leave it in the end of September. In the con-

cluding portion of Part I., Dr. Clark protests loudly against the practice of sending patients, in a late stage of consumption, abroad. He details several striking and melancholy instances of this cruel measure from the stores of his own experience. The following extract does equal honour to his understanding and his heart; we earnestly call the attention of our readers to it.

"During my residence on the continent, I have had frequent occasions to remark with surprise, the very advanced stages of the disease in which many of our consumptive patients were sent abroad. This is the more remarkable, as, however medical men may differ about the propriety of sending such patients abroad in the earlier, there surely ought to be no question about its impropriety in the latter stages. For my own part, I have seen enough to convince me, that it is not only a very useless, but often a very cruel thing, to banish such patients from all the comforts of home, and send them forth to undertake a long journey through a foreign country, deprived probably of all they hold dearest to them, and without those thousand nameless comforts by which the watchful care of friends may cheer even the last period of a hopeless disease. The medical man who reflects on the distresses that such patients must be liable to during such a journey, arrested perhaps in their progress by the increase of some of those symptoms which attend the advanced stages of consumption, in very indifferent accommodations probably, and far from any medical advice in which they can confide, will surely long hesitate ere he condemns the fated victim of this remorseless malady to the additional evils of expatriation: and his motives for hesitation will be increased, when he considers how often the unfortunate patient sinks a prey to his disease, long before he reaches the place of his destination; or at best, arrives at it in a much worse condition than when he left England, and doomed, shortly, to add another name to the long and melancholy list of his countrymen that have sought out, with pains and suffering, a distant country, only to gain in it an untimely grave!" 117.

In dwelling on the highly important subject of the effects of foreign climates on consumption, we have carried our ex-

tracts to such a length as to prevent our noticing many other interesting matters in Part I., especially a capital memoir on *Cretinism* and *Goitre*, and a report on the *Malaria Fevers of Rome.* The same circumstance makes us content ourselves with a few extracts only from Part II. This part consists of Notes on several of the Parisian Hospitals, and on the Hospitals and Medical Schools of Lyons, Strasbourg, Turin, Genoa, Pavia, Padua, and Bologna. Of the hospitals of Paris he notices La Charité, St. Louis, Maison des Enfans Malades, Hôpital Necker, Val-de-Grace, Salpetriere et de Clinique Interne. In his notes on La Charité, he confines his remarks chiefly to the practice of Dr. Esquirol, in giving the nux vomica in paralysis. This seems to be a powerful but very dangerous remedy. Several patients have been killed by its use. The following extract from the account of the Maison des Enfans is interesting, and we may add, is very flattering, by way of contrast to the practice in this country.

"In the spring of 1818, small pox was very frequent in Paris, and very fatal; more than half of those admitted into this hospital sometimes dying.

"Strange to say, the heating and sudorific plan was the one adopted at this time in the treatment of these little patients! All those who died were examined, and the appearances observed were inflammation and ulceration of the internal coat of the intestines, pustular eruptions there, or on the surface of the peritoneum; and, in three cases, a false membrane, similar to that of croup, was found lining the whole alimentary canal from the œsophagus to the rectum: in these three cases, the eruption on the surface had never shown itself distinctly. These morbid appearances called the attention of the medical officers to Dr. Broussais' opinions; the cooling antiphlogistic treatment, with the application of leeches over the abdomen, was adopted in consequence, and the speedy reduction of the mortality soon evinced the superiority of the practice.

"The following statement of the proportion of deaths from this disease is interesting on many accounts; and taken in conjunction with the circumstance just detailed, cannot fail to make a strong impression on the mind of the English reader.

"*Small Pox Cases admitted into the Hospital des Enfans Malades, in* 1816, 1817 *and* 1818.

		Cured.	Died.	Total.
1816		42	53	95
1817	[From Jan. to Aug.]	51	37	88
1818		117	102	219
		210	192	402

Page 150.

In his account of L'Hôpital Necker he dwells chiefly on the discovery of Dr. Laennec, on which we shall say nothing, having said so much in our last number. We are happy to see our opinion of the utility of Dr. Laennec's diagnostics corroborated by that of so intelligent a practitioner as Dr. Clark.

In his notes on the Val-de-Grace, he gives an account of the doctrines and practice of Dr. Broussais, an exposition of which we expect soon to lay before our readers in detail. We cannot help remarking, however, at present, that, like most theorists, this eminent physician is too exclusive and confined in his mode of explaining morbid action.

In almost every disease that affects the human body, Dr. B. can see nothing but inflammation of the mucous membrane of the stomach and intestines.

Dr. Clark appears to entertain but an indifferent opinion of the *practice* followed in the Parisian hospitals.

"The practice appeared to me particularly inert: ptisanes pectorales, eau gommée, lavement emollient, &c. were among the most active prescriptions, and few escaped without some one or two of these. 'Tis sickening to an English physician, in visiting the hospitals of Paris, to hear nothing but these eternal ptisans ordered for every patient, let the disease be chronic or acute. It may be said they are harmless at any rate, and may satisfy the patient that something is doing. But surely the latter consideration is little necessary in hospital practice, and I am inclined to think that the plan is often worse than harmless, as it operates, in some degree, on the mind of the physician, and leaves an impression that he has done something, when, in truth, he has done nothing. In this

way, perhaps, it prevents the employment of more active means when they are actually requisite." 170.

He seems to us to account pretty well for this state of things.

"Generally speaking, the physicians in the hospitals of Paris, as appears to me, have too many patients to attend. Though it was summer when I last visited them, and there were extremely few fevers and acute diseases, yet Dr. Fouquier, of La Charité, had one hundred and four patients under his own charge, with one assistant only. Dr. Recamier, of the Hotel Dieu, had one hundred and ten, and the physicians of St. Louis had a still larger proportion.

"The fatal cases are generally examined after death. Dr. Fouquier told me, that for twelve years that he had been physician to La Charité, no patient had died without being examined.

"It is allowed, that post mortem examinations, when properly conducted, form one of the principal means of enabling the physician to acquire an intimate knowledge of the nature of disease, and to lead to those principles which are to guide him in the treatment of it. But the knowledge derived from these examinations, with whatever care they may be performed, when taken by itself, is but of little real utility. If the previous history of the disease be not accurately known, little is to be expected from them; and it is a deficiency of this knowledge that appears to me the principal circumstance which has rendered these examinations at the hospitals of Paris of so little real value. I have attended not a few of these examinations, and seen the surprise excited at some appearances, which, had the morbid phenomena during the course of the disease been carefully observed, must have been known at least, if they could not have been remedied; and I have also had occasion to remark the difference that sometimes occurred between the physician and assistants with respect to previous symptoms. All this convinced me how truly useful these researches might have proved, but for the want of information relative to the history and progress of the disease. This circumstance, however, certainly cannot be sufficiently at-

tended to, where the physician has so many patients under his care." 130, 131.

An interesting and full account is given of the hospitals at Lyons. It appears that the antiphlogistic mode of treating febrile disease, is making gradual progress here also, and not before it is time, since Dr. Clark informs us, that *even now*, four-fifths of the practitioners in this city are Brunonists! We quote the following statement in justice to our continental brethren.

"In this gentleman, Dr. Desgaultières, I found a very enlightened physician, who had had much experience, both in the army and in private and hospital practice; and, in the present unsettled state of the opinions of medical men about the cause of fever, his may be stated here with propriety. I do this the more readily, as it appeared to me that his opinion was formed, not from any theoretical notions, but from careful and extensive observation, and because it approximates so closely to that which the great increase of pathological researches has led so many British practitioners of late to adopt. His idea is, that fever always depends upon local affection, irritation or inflammation of some organ, or even an unusual accumulation of blood, congestion, in some viscus, without absolute inflammation. He is far from limiting such local affection to the gastro-enteritic system with Dr. Broussais; yet his conviction is, that fever invariably depends upon, and has its origin in some local affection, and that from this we ought to set out in our explanation of its phenomena." 183.

Dr. Clark's account of the medical school of Strasbourg is extremely meagre. Its prosperity seems on the decline; "a few years ago, the number of medical students amounted to from four to five hundred; at present, there are only about two hundred, and from thirty to forty only take their degrees annually." The Italian schools seem somewhat more flourishing. Among these, Bologna ranks as one of the first.

"Medicine at Bologna is cultivated with assiduity, and the professors are men of established reputation. Tommassini, professor of Medicine, is well known as the author of an excellent work on yellow fever, and several others of less note. He is one of the great supporters of the new Italian, or con-

tra-stimulant doctrine, as it is called; and which, in practice at least, is directly opposed to the system of Brown, which for a long time was advocated warmly in Italy. The latter, it is well known, considered almost all diseases as arising from debility, and requiring stimulants, while the present doctrine, 'Nuova Dottrina Medica Italiana' looks upon most diseases as arising from increased action, and requiring depriments—contra-stimulants. The medical school of Bologna may be considered at the head of this new doctrine, and the professors its most able and zealous advocates." 193.

Dr. Clark gives a full detail of the plan of medical study prescribed by the university, which seems very judicious. The course extends to four years.

"The mode of examination of candidates for the different degrees seems very judicious, and is considerably different from that followed in our medical schools. Five professors constitute a quorum; and at the examination of a candidate, each submits to him twenty different subjects, taken from his own particular course of instruction. He draws one of these by lot, and is forthwith examined upon it, whatever it may be. At the termination of the examination, the professors give their votes secretly, and the number of these obtained, and the mode in which they are obtained by the candidates, constitute three different modifications of the same degree, which are expressed in the diplomas.

"If they obtain two-thirds of the votes they are declared *approved;* if the whole, they are declared *unanimously approved*, a pleni voti; and to express a still further praise, they are declared *honourably approved*,—approvati conlodi. Those who are rejected cannot present themselves till after another year's study.

"I was told that the plan of study just described is about to be changed, by an order from Rome." 199.

"There are altogether about two hundred medical students at the university of Bologna; but of these, a small proportion only aspire to the higher ranks of the profession. Many, I found, were merely preparing themselves for being apothecaries, and many to practise the lower branches of surgery—'Bassa Chirurgia,'—a class in the profession unknown among

us, and holding a rank something similar to the 'officiers de santé,' in France.

"This system of sending forth a set of half educated men, who, no doubt, as in other countries, overstep in their practice the limits prescribed to them, is, with justice, complained of by the regularly educated members of the profession in Italy. The Germans are blamed for introducing this practice. A system more calculated, indeed, to bring the profession into disrepute, cannot well be imagined; and it may be one, among several other causes, of the very different rank in society which the medical men of Italy hold, compared with those in England. But a still stronger reason may be found, I apprehend, in the system of gratuitous medical education in use in the former country. The students attend the lectures and hospitals almost for nothing, and the graduation fees are very trifling. The annual salary of the medical professors themselves, amounts only to about 150*l.* The facility thus afforded to medical students of acquiring their profession, on easier terms than the lowest mechanic can his trade, so common on the continent, and of which they boast, though it may have some advantages, is, upon the whole, I am inclined to believe, injurious to the profession." 196.

"*Padua.* The university of Padua at present holds a deservedly high character in Italy, as a medical school. Within the last few years, the number of students has considerably increased, and the present building being found too small, the emperor, on his late visit to Italy, gave orders for the erection of a new one, on a more extensive scale. The plan of medical studies in the university of Padua is good. It extends to five years; and two courses of lectures are delivered in each session." 204.

The following extract respecting Pavia will be interesting to all who venerate, and who does not venerate, the name of the great man, with whose name that of his city must be eternally associated in the memory of the votary of medicine.

"Every thing about the medical school of Pavia is very perfect, and the musea, class rooms, &c. all arranged and fitted up in a very superior style; yet I was surprised to find every thing on so small a scale at a university, which, at one time

collected so great a concourse of students from all parts of Europe, and boasted of some of the first medical professors in the world.

"As a medical school, Pavia is now much fallen in estimation. The number of medical students of all classes in the session of 1817 and 1818, I was told, amounted to nearly four hundred; and that about forty annually took the higher degrees of the profession. The wards of the general and clinical hospital were in good order, but small. We must not, however, forget, that the town is but small, containing only about 20,000 inhabitants. This a circumstance which must always have been against Pavia as a school of practical surgery. It ought, however, to add not a little to the merit of the venerable Scarpa, whose works have added so largely to the stock of surgical knowledge, and to whom this university is so much indebted.

"Scarpa is now the director of the medical department of the university; the plan of medical study at which, I shall not mention, as he informed me they are about to change it. Scarpa's sight has failed him much, yet he appears to enjoy good health, and a flow of spirits and cheerfulness not often met with at his age. Although in a great measure retired from the exercise of his profession, he has lost nothing of his enthusiasm for it. In his library, I found all the best English medical works. He complained much of the very tedious manner in which he received publications from our country, being obliged frequently to get them round by Germany.

"Whoever visits this great man will, in addition to the pleasure and information derived from his animated conversation, find in his house a collection of some of the finest paintings by the first Italian masters; for Scarpa is an admirer and an encourager of the fine, as well as the useful arts." 210.

The whole of the establishments in the Sardinian dominions seem to wither beneath the deadly sway of an ignorant and despotic government. We shall conclude this article by the following most horrible account of the lunatic asylum, Casa de' Pazzi, of Turin.

"My expectations of this establishment were not raised very high, from the state in which I found that which I have

just mentioned, and still less from some conversation I had with one of the medical professors of the university the evening preceding my visit, during which I perceived evidently a wish that I should not see it; but I never could have anticipated the dreadful spectacle of tortured humanity which was here exhibited, and of which any description I can give must come far short of the impression it made upon my mind. I shall simply state the situation of the wards and patients as I found them at the time of my visit.

"The part of the hospital we were first taken to, consisted of small rooms, similar to those generally met with in such institutions, but I was disappointed to find these were not for the poor patients, but for those who paid a certain sum for being kept. The first of these that was opened presented to us the wretched prisoner perfectly naked, and chained down to his bed by both wrists. He had raised himself up in his bed, as far as his chains admitted, by which movement he had thrown off the single coverlet that had been cast over him. He had no shirt; his legs, apparently red and swollen from cold, were drawn up under the corner of the bed cover, which lay over a small part of his body; he was pale and emaciated; he uttered not a word. In short, a human being in so wretched a state I had never before seen; but I was soon to witness others in a state still more horrible.

"We were next conducted into a ward where thirty beds were huddled together, on most of which lay a poor wretch chained by one or more limbs to the bedstead; for to each corner of these was attached a massy iron chain, with a clasp of the same materials and strength at the extremity, for admitting the wrists and ancle; and, according as the keeper judged necessary, one or more of these were applied. Some were polished, and bright as silver, from constant use. I imagined these were their most unruly patients, but was told that this was by no means the case.

"To these we were next led; and on the unbolting of the door of a large cell, the scene that presented itself almost exceeds belief. The spectacle of the poor wretches, naked, or covered only by some straw, chained down hand and foot to their bedsteads, the clanking of their chains, the dreadful vo-

ciferations they set up at the sight of him who riveted these chains, and still more, the horror excited by such a spectacle, no terms are strong enough to depict! I had read and heard of chains and other means of torture for subduing, [irritating] the unfortunate maniac; I had even seen such singly chained to the wall by the neck, like an infuriated and dangerous beast; but a den like this, crowded and crammed with human beings, chained down, without a rag of covering, struggling to raise their heads, and exhibiting their emaciated and galled limbs from the heap of straw that had been thrown upon them, was a scene I never expected to witness, and which I hope in God I may never witness again! After visiting similar establishments in Paris, and some in our own country, where chains have been laid aside, and a mild system of treatment adopted, with a success which, setting humanity out of the question, more than warrants its continuance, I was ill prepared to meet such a dreadful contrast on crossing the Alps. In this cell there were twelve men, three of whom only were allowed any thing more than straw to cover them. Some I was told had been confined there for many months. On approaching them, they exhibited their chained limbs with the most earnest entreaties for liberation. One man had two chains on one arm. In this case, the space between the iron clasps was red, swollen, and ulcerated, and the mortification, which, in all probability was to follow, would soon render chains unnecessary for him. Others had their limbs galled, but not in such a degree as that described. In one instance only, in the whole hospital, did I observe any thing introduced between the iron ring and the limb. The rest of the men's wards were similar to that I first noticed.

"From the men's we were led into the women's department, which was in the higher part of the house, and which, in every respect, we found similar to that we had just left; the beds huddled closely together, chains always ready, many applied, and most of the beds occupied; for, whether to save trouble, or from the poor creatures having no clothes but the coverlet that was thrown over them, almost every one was in bed. Here, as below, was also a cell, where straw afforded

the only covering, where the chains were more heavily applied, and where the state of furious desperation to which the wretched victims were driven, was expressed in terms equally violent and still more affecting. One of these tortured women held up her arm, which was raw and had been bleeding, from the iron clasp having worked its way into the flesh!

"Such is the dreadful state of this house, which contains one hundred and eighty males and ninety-seven females; of whom one-third, the keeper told me, were kept *constantly* chained. From the same source I learned, that the annual number of deaths, and this I apprehend is the principal way in which this house gets rid of its inhabitants, sometimes amounted to eighty, nearly one-third of the whole; that they *had* been as few as thirty; and that the average was fifty, nearly one-fifth—a mortality I believe unequalled in any institution of the kind in any country.

"With respect to the murderous receptacle for the deranged, I know not what relief to propose; nothing but a perfect change, I fear, can remedy the state it is in; perhaps it would even be for the benefit of humanity, that the same dreadful remedy were applied to it as was formerly applied to the Hotel Dieu of Paris; and could it rise from its ashes in *Piedmont*, as that hospital did in France, he who applied the torch would deserve to be ranked among the benefactors of his country and mankind. In its present state, it is an infamous disgrace to the government under which it exists: I say government, because, in a despotic one, such as that of Piedmont, where private opinion dares not be expressed, the government alone is responsible for the state of all such institutions. But these things are beneath the notice of a king of Sardinia, who, by the way, is particularly bound to visit La Carita in person; but he prefers a deputy, who again, I fear, employs a substitute in imitation of his magnanimous sovereign. The royal family of France are not ashamed, in their visits to the departments, to look into the hospitals, and even to administer consolation at the bed-side of their subjects; and the emperor of Austria, in his late journeys, visited the hospitals, as well as other public institutions: but such is the horror of an hospital at the court of Sardinia, that a physician who entered one, is not suffered

to cross the precincts of the palace, lest he should carry the seeds of infection among the royal family!" 223.

After this ample exhibition of interesting specimens of Dr. Clark's volume, in presenting which to our readers, we have endeavoured to minister to their gratification, while we offered a fair view of the work itself, it is hardly necessary for us formally to express our opinion on its merits. These certainly are very considerable, but they are far from being without alloy. The greatest fault of the work is its brevity. Of some very interesting scenes, so very little is said, that we cannot easily acquit the author of negligence, or want of curiosity, during his visits to them. He, indeed, tells us in his preface, that he professes to give us only "detached notes;" and seems to consider, that we ought to be thankful for what he gives us. We ARE thankful; yet, we repeat, that we would have been more satisfied had he given us *no* information respecting some of the places, than we are by the meagre morsels which he has vouchsafed to give us from the rich and overflowing granaries to whose half-opened portals he conducts us. We trust, however, that before Dr. Clark finally revisits his native country,* he will be enabled to fulfil the hopes expressed in the preface, of filling up the outline which he has chalked out in the present essay.

"At all events," he says, "this essay will be useful to myself, as a sort of text book for future observations of a like nature, and as a basis whereon, I hope, more extensive opportunities may enable me to rear a superstructure more creditable to myself, more commensurate with the importance of the subject, and more worthy the attention of the public."

* Dr. Clark, we understand, is at present residing at Rome, of which city he is preparing a Medical Topography, which cannot fail to be highly interesting.

Pharmacologia; or, the History of Medicinal Substances. By John Ayrton Paris, M. D. F. L. S. Third edition. 8vo. London, 1820.

[From the London Medical Repository, for May, 1820.]

We hail the appearance of a third edition of this work as a proof of the greater attention to the study of the materia medica and pharmacy than was usual a few years ago. Notwithstanding the reveries of the old chemical physicians, respecting the existence of one single universal medicine which should cure all constitutional diseases, had long passed away like the baseless fabric of a vision, yet there still existed in the minds of the majority of the profession an undue propensity to rely upon the exhibition of mercury, the modern panacea, iron, antimony, bark, ardent spirits, and a *few* other potent remedies, in different doses, rather than upon the skilful use of a more varied materia medica; a preference which was in some measure cherished by the legal necessity imposed on the great majority of actual practitioners of furnishing medicines to their patients, and their consequent wish to make as small a stock as possible, and especially of perishable articles, serve their purpose.

It may, perhaps, be said, that this preference of a paucity of drugs was also owing, in part, to the want of sufficient chemical knowledge as to the compounding of a more varied list, from an apprehension of introducing substances which might have the effect of neutralizing or even altering the intended action of the main ingredient; especially as the old dispensatories afforded but a slender guide in this respect. Pharmaceutical chemistry has, however, received so much illustration from the labours of the German and French apothecaries, that the compatibility or incompatibility of substances with one another in the same formula are well ascertained.

Still, however, there remained a want of a scientific treatise in our vulgar tongue respecting the mode of compounding medical formulæ, or the art of prescribing; and it is to supply this want that Dr. Paris has principally directed his attention. Not that he neglects the other parts of pharmacy, usually giv-

ing under each article of the London pharmacopœia, to which alone he very properly confines his attention, and the materia medica and preparations of which he has incorporated into a single alphabetical series, its sensible qualities, its chemical composition, its relative solubility in different solvents, and the proportions in which it should be mixed or combined with different bodies, in order to produce suspension or saturation; the substances that ought not be mixed with it, the most eligible forms in which it can be exhibited, the proper doses to secure specific effects, these medicinal uses and effects, the officinal preparations of the simple substances, and, lastly, the usual adulterations. As he notices all these in a most distinct and satisfactory manner, it may easily be conceived, that this work forms a most excellent manual for the practitioner of medicine; and from a perusal of it, we can recommend it in the strongest terms, not only to the junior members of the profession, but even to the most experienced, who will find in this volume many new observations.

The work is preceded by an historical introduction, the substance of four lectures delivered by the learned author to the Royal College of Physicians, on the Philosophy of the Materia Medica; in which he investigates the causes that have in different ages operated in producing the revolutions that characterize the history of medicinal substances; namely, superstition, credulity, scepticism, false theories, devotion to authority, and established routine, in which he gives an account of the mischievous interference of the college of Paris, in regard to the use of antimony, the assigning to art that which is the effect of unassisted nature, the ambiguity of nomenclature, the progress of botanical science, the application and misapplication of chemical philosophy, the influence of climate and season on diseases, as well as on the properties and operations of their remedies, the fraudulent adulteration of medicines, the unseasonable collection of plants, and the obscurity which has attended the operation of compound medicines.

We perfectly agree with Dr. Paris, in his observations on the frequent changes of names which the colleges have adopted from the philosophical chemists. His words are:—

"I am unwilling to join in the commendations which have

been so liberally bestowed upon our chemical nomenclature; nay, I am disposed to consider it as a matter of regret, that the names of our medicinal compounds should have any relation to their chemical composition; for, in the present unsettled state of this science, such a language must necessarily convey theory instead of truth, and opinions rather than facts; in short, it places us at the mercy and disposal of every new hypothesis, which may lay our boasted fabric in ruins, and, in its place, raise another superstructure, equally frail in its materials, and ephemeral in its duration. Thus CORROSIVE SUBLIMATE was a *muriate* of mercury, or an *oxymuriate*, until Sir H. Davy established his new theory of chlorine, and then it became a *bichloride*. At some future period chlorine will be found to be a compound, and then it must have another name: for the same reason the term CALOMEL is surely to be preferred to *submuriate* or *chloride*. TARTARIZED ANTIMONY again has been called by our nomenclatural reformers, the *tartrate of antimony and potass;* but is it a triple compound? Gay Lussac thinks not, and considers it as a combination in which *cream of tartar* acts the part of a simple acid. The French, in their new *codex*, are still more extravagant in their application of chemical nomenclature; thus, the subcarbonate of potass is called by them the *sub-deuto-carbonas potassii*. The first part of this quadruple name indicates the comparative quantity of acid in the salt, the second that of oxygen contained in the base, the third announces the acid, and the fourth the basis of the base."

Although we conceive it of little consequence, yet we cannot but imagine that Dr. Paris is in an error in attributing the origin of alchemical notions to the Arabs. In our opinion, those notions first originated in Egypt, among the Christian clergy: this appears evident, from the high ecclesiastical rank of the principal Greek authors, several of whom were anterior to the age of Arabian learning, and also from the religious mysteriousness of their writings; whereas the Arab authors, as Geber, are remarkable for their clearness; and, although they give processes for the production of apparent gold and silver, yet acknowledge the imperfection of them, and discuss a number of theories, by the reduction of which to practice it

had been judged possible to produce perfect metals; so that nothing can be more erroneous than deducing the etymology of the word gibberish, from Geber, as stated by Dr. Paris, from Dr. Johnson.

Another trifling mistake in the history of chemistry refers to the name of the great Coryphæus of the chemists, the redoubted Paracelsus. Dr. P. informs us, that he termed himself Philippus Theophrastus Bombastus Paracelsus de Hohenheim; but although here are names enough even for a person of no ordinary abilities, yet one is omitted, and the order of the others disturbed; his name, as stated by himself, being Aureolis Phil. Par. Theoph. Bomb. de H. As the activity and violence of this man were the means of rousing the profession from the study of the old Greek physicians to that of nature, and the origin of our modern improvements, so we ought not to detract either from his name or his merit.

In the second part of this introduction, Dr. Paris investigates the objects to be obtained, and the resources which are furnished, by medicinal combination, according to the following order:—

"*Object* 1. To promote the action of the basis, by combining it with substances which are of the same nature, *i. e.* which are individually capable of producing the same effects, but with less energy than when in combination with each other; or by combining the basis with substances of a different nature, and which do not exert any chemical influence upon it, but are found by experience, or inferred by analogy, to be capable of rendering the stomach, or system, more susceptible of its action.

"2. To correct the operation of the basis, by obviating any unpleasant effects it might be likely to occasion, and which would pervert its intended action, and defeat the objects of its exhibition.

"3. To obtain the joint operation of two or more medicines, which have different powers, and which are required to obviate different symptoms, or to answer different indications.

"4. To obtain a new and active remedy not afforded by any simple substance, either by combining medicines which excite different actions in the stomach and system, in conse-

quence of which, new or modified results are produced; or by combining substances which have the property of acting chemically upon each other, the results of which are the formation of new compounds, or the decomposition of the original constituents, and the development of their more active elements; and also by combining substances, between which no other chemical change is induced than a diminution or increase in the solubilities of the principles in which their medicinal virtues reside.

"5. To afford such a form as may be efficacious, agreeable, or convenient."

In that great and important question, Whether any chemical proofs of the presence of white arsenic, short of its actual reduction to the state of metal, can be depended upon, or ought to be received as evidence in the courts of criminal law? Dr. Paris says,

"That after a full experimental investigation of the subject, and after an impartial review of all the facts which bear upon the question, the author feels no hesitation in declaring it to be his conviction, that white arsenic may be detected without any fear of fallacy by a proper application of tests, and that the contrary opinion is entirely founded in error."

Now, although we may accede to this proposition, when the examiner is a professed student of chemistry, and versed in its manipulations, we cannot by any means agree to it in cases when the examination is made by a person who has not bestowed continual attention on that science, but who has merely studied it slightly as a subordinate branch of medicine, or, perhaps, of pharmacy; and, therefore, as the increasing number of books on medical jurisprudence will bring, of necessity, the subject of poisoning continually before the public eye, and we may, of course, expect a great increase of that horrid crime, and a greater skill in exhibiting deleterious substances; so we fear, on the other hand, that a desire of appearing to possess more knowledge of chemistry than they really do, will induce many to rest satisfied with slight indications, to give decisive evidence in doubtful cases, and thus bring many innocent persons to a shameful death. We therefore think, contrary to Dr. P., that as it is better that ten guilty

persons should escape than a single person, or, to speak more properly, family, as all are implicated in the shame, should suffer, so nothing but an actual procuring of the deleterious article should be regarded by a medical person as warranting his affirming the party to be poisoned by it; and that the use of pretended tests should be regarded only as preliminary experiments to point out the method of obtaining the arsenic, or other poisonous substance.

In the examination of elaterium, a substance whose action on the human body is still more violent than arsenic, Dr. Paris has discovered a new vegetable principle. The active principle of this plant is lodged only in the juice around the seeds, and in so small a quantity, that Dr. Clutterbuck (MEDICAL REPOSITORY, Vol. XII. No. 67,) obtained only six grains from forty cucumbers; and even of this fecula, or feculence, as Dr. Paris proposes to call it, although an eighth part of a grain will purge violently, yet not more than one-tenth of this virulent substance possesses any active virtues. As Dr. P.'s experiments are new, and a very excellent supplement to Dr. Clutterbuck's paper, we shall insert them at full length:—

"Experiments.—Series the First.

"A. Ten grains of elaterium, obtained from a respectable chemist, and having all the sensible properties which indicated it to be genuine, were digested for twenty-four hours, with distilled water, at a temperature far below that of boiling; *four grains* only were dissolved.

"B. The solution was intensely bitter, of a brownish yellow colour, and was not in the least disturbed by alcohol, although a solution of *iodine* produced a blue colour; the solution, therefore, contained no gum, and only *slight traces* of starch.

"C. The solution, after standing twenty-four hours, yielded a *pellicle* of insoluble matter, which, when burnt, appeared to resemble *gluten*.

"D. The six grains which were insoluble in water were treated, for forty-eight hours, with alcohol, of the specific gravity .817, at 66° of Fahrenheit; a green solution was obtained, but by slow evaporation *only half a grain* of solid green matter was procured. The insoluble residue obstinately adhered

to and coated the filtre, like a varnish, and completely defended the mass from the action of the alcohol: it is probable that it consisted principally of *fecula*.

"Experiments.—Series the Second.

"E. Ten grains of elaterium, from the same sample, were treated with alcohol, of the specific gravity .817, at 66° Fahrenheit, for twenty-four hours; upon being filtered, and the residuum washed with successive portions of alcohol, the elaterium was found to have lost only 1.6 of a grain. The high specific gravity of the alcohol in this experiment was important; had it been lower, different results would have been produced.

"F. The alcoholic solution, obtained in the last experiment, was of a most brilliant and beautiful green colour, resembling that of the oil of cajeput, but brighter: upon slowly evaporating it, 1.2 grains of solid green matter were obtained.

"G. The solid green matter of the last experiment was treated with boiling distilled water, when a minute portion was thus dissolved, and a solution of a most intensely bitter taste, and of brownish yellow colour, resulted.

"H. The residue, insoluble in water, was inflammable, burning with smoke, and an aromatic odour, not in the least bitter; it was soluble in alkalies, and was again precipitated from them unchanged in colour; it formed, with pure alcohol, a beautiful tincture, which yielded an odour of a very nauseous kind, but of very little flavour, and which gave a precipitate with water; it was soft, and of considerable specific gravity, sinking rapidly in water; circumstances which distinguish it from common resin; in very minute quantities it purges. It appears to be the element in which *all* the powers of the elaterium are concentrated, and which have been denominated *elatin*.

"I. The residuum, insoluble in alcohol, weighing 8.4 grains, (Exp. E.) was boiled in double distilled water, when 5.9 grains were dissolved.

"J. The above solution was copiously precipitated *blue*, by a solution of *iodine*, and was scarcely disturbed by the *persulphate of iron*.

"K. The part insoluble, both in alcohol and water, which was left after experiment I, amounted to 2.5 grains; it burnt like wood, and was insoluble in alkalies."

From these experiments, Dr. Paris expresses the chemical composition of elaterium in the following manner:—

I.	F.	Water,	.4
	B.	Extractive,	2.6
	B. D. J.	Fecula,	2.8
	C.	Gluten,	.5
	K.	Woody matter,	2.5
	H.	*Elatin*,	1.2
	G.	Bitter principle	
			10

The bitter principle in elaterium is very distinct from its extractive matter; the solution obtained in Exp. G. being diluted and swallowed, produced only an increase of appetite; and the solution B. produced no effect whatever.

Dr. Paris mentions the most generally used nostrums with the due abhorrence of medicines whose preparation, being unknown, are, of course, liable to be prepared differently by different persons, however well-intentioned they may be, for want of some certain standard; but we are surprised to see one excepted from this merited proscription, at p. 260, whose name we omit, not wishing to be instrumental in giving it publicity, where he not only informs us that the medicine "*is admirably adapted for domestic use*," but also obligingly gives the names and residences of the proprietor and his confidential agent. We know not how to account for this departure from his accustomed tone respecting these arcana, having too high an opinion of Dr. Paris to suspect, even for a moment, any interested motives for this lenity, and, what is more, praise.

He mentions, in his preface, Mr. Barry's patent extracts, prepared *in vacuo*, and conceives, *à priori*, that the results must be more active than those obtained in the ordinary way. Respecting this subject we may observe, that having examined some of these extracts, we find that of belladonna inferior in smell to the common; and, also, that the extract of senna be-

came speedily covered with mould, and those of papaver and taraxacum are beginning to grow mouldy, although not the least mouldiness is to be found on the common extracts of the same plants, which were obtained before the others, and all the pots have been kept in the same drawer. It would appear, therefore, that the heat used in preparing these extracts, *in vacuo*, is not sufficient to destroy the vegetative power of the seeds or sporules of the fungi adhering to the plants.

BIOGRAPHY.

FOR THE ECLECTIC REPERTORY.

Biographical Notice of Dr. La Roche.

Died on the 6th of June last, in the sixty-fifth year of his age, Rene La Roche, M. D. Member of the Royal Society of Medicine of Madrid, Honorary Member of the Medical Society of New Orleans, &c., formerly of the Island of St. Domingo, and for many years a highly respected practitioner of this city.

In recording the death of this truly amiable and learned physician, but whose extreme reserve and modesty had in a great measure limited his sphere of usefulness and consequently his reputation to the circle of his countrymen, we pause for a moment at the reflection which naturally suggests itself, of the wide difference between the fame that survives the man of practical utility alone, and that brilliancy which ever surrounds the funeral car of the medical professor. The society in which we live, or perhaps it would be more correct to say, mankind at large, so few of whom are competent to decide on the merits of medical men, are naturally led to forget the claims of the useful and laborious practitioner, amidst the bursts of eloquence from the medical rostrum or the genius and talents displayed in the writings of a Rush, a Barton, or a

Wistar. The fame of such teachers, and of such writers, is undoubtedly deserved and should be cherished with reverence,—but when we do so, do we perform all our duty? Does not the following their precepts for the relief of suffering man, constitute the greatest claims of all physicians to the gratitude of society? It is on this account that the memories of those friends of mankind throughout the world, who leaving aside the fascinating paths of literary and ambitious preferment, have sacrificed the enjoyments of their whole lives to the tedious and painful labours of suffering humanity, should not perish with their mortal remains. Justice and gratitude require that we should not allow the examples of the benefactors of the human race to sink into the oblivion of the tomb; but on the contrary, that we should treasure up their remembrance in our hearts and hold them up as models to the rising generation.

If a long life devoted to the relief of the poor and sick, if the practice of those virtues, which conciliated the esteem and won the affections of all who knew him, can entitle one man to the gratitude of his fellow men, our departed friend carried with him to another and a better world, the regrets of the worthy and the just. But those who have survived him—those whom his friendship and intimacy had honoured while a sojourner in this valley of tears, have one more duty to fulfil;—a duty not more required by departed worth, than by the society he so long contributed to comfort by his example and by his life.

Réné La Roche was born of respectable parents in the Island of St. Domingo in the year 1755. According to the customs of the wealthy inhabitants of the country he was sent at an early period to France, to receive his collegiate education. Having completed his classical studies, he felt desirous of becoming a disciple of Hippocrates, and persevering in his intention, he soon inscribed his name among the students of the then celebrated university of Montpellier. As a medical school, Montpellier at that period had no rival, except that of Edinburgh, whether by the number of its students or by the talents and acknowledged fame of its professors. Lamure, Barthez, Leroy, Vigaroux, Broussonet, &c. were his teachers; and from these fathers of the art, he received, with his medi-

cal instruction, that persevering spirit of observation, which distinguish him through life. Under these masters he could not fail becoming ardently attached to the profession of his choice; and after going through the usual trials with credit to himself, he received his diploma in 1779.

The zeal and ambition so natural to his age, urged him to commence the practice of his profession as soon as he returned from Europe in 1780. Possessed of solid instruction, well grounded in the principles of the science of medicine, and endowed with a strong and reflecting mind, his claims to public patronage were not overlooked;—an extensive circle of patients soon bestowed upon the youthful practitioner their full confidence, and in the course of a few years, fortune, often blind, but sometimes clear sighted, had showered upon him her choicest favours,—but alas! how futile are her gifts, how delusive her pleasures. The monster revolution showed its hydra head in this favoured and remote spot, and war, havoc, and ruin desolated another Eden, a land where happiness had fixed her abode. The misguided sons of Africa, in the fervor to recover their freedom, gave way to every excess, and fire and the sword became the instruments of destruction to proprietors and property. It is too fatally engraven in the hearts of many who may chance to peruse these lines, that a general massacre of all who could not defend or save themselves, soon swept from the face of this devoted island every one of its white inhabitants. But in the midst of this horrific transaction, while blood was streaming from every pore, some latent sparks of humanity still existed in the hearts of these deluded men. Gratitude, though suppressed, was yet alive;—it impelled them not only to preserve, but even to defend the lives of those physicians, who in fulfilling the duties of their profession, had used the language of kindness and protection. Dr. La Roche and some of his surviving colleagues and friends now in this city, were of this number.

Having thus escaped the fury of these men of blood, Dr. La Roche with many of his acquaintances reached the friendly shores of this country in 1793, where they experienced that liberal hospitality which distinguishes the American character. The fatal epidemic which at that time desolated this conti-

nent, soon brought into notice the French physicians, and the success that attended their mode of treatment justified the attention of the inhabitants of Philadelphia. And without detracting from the high claims of the practising physicians of that period, we may be allowed to state, that their colleagues from St. Domingo were equally fortunate in the treatment of the yellow fever. Let it not be urged that they derived from a previous acquaintance with the disease, the experience necessary to counteract its effects, for we know from the most unequivocal and honourable testimony, that until they had reached the American continent, they were utter strangers to this devastating form of disease. It would, however, savour of partiality were we not to concede, that the bilious remittent form of fever which prevails so often in the West Indies, had furnished them with results by which they were guided in the treatment of our yellow fever; and how judiciously, can be attested by numerous surviving and grateful individuals.

From that period our revered friend became a resident of this city, and as it continued to be the place of refuge of many of his countrymen, he soon found himself fully employed in his profession. Among the victims of that unhappy period his philanthropy and his benevolence found ample scope for their exertion. Nor were his useful talents confined to that very respectable portion of our inhabitants. During twenty-seven years of laborious practice, he was often called to visit American as well as French families; and though a stranger to the language as well as to the habits of the former, he gained their confidence and commanded their respect.

Dr. La Roche had always enjoyed a feeble state of health, which the labours of his profession by no means contributed to fortify. Perhaps also the vicissitudes of our climate had aggravated it; for he was subject during the latter years of his life to attacks of lumbago, which though not very severe, required much care and attention. He was just recovering from one of these attacks, and had been enabled for a few days to walk out, when he was thrown by accident in the street on the 2d of June, and received a severe contusion on the parietal bone. Conveyed senseless to his house, he never recovered to their full extent, the exercise of his faculties, but after various

gleams of returning reason and of proportionate hope to the crowd of friends that thronged his door, closed, on the 4th day of the accident, his valuable life. It would be useless to add, that the illness of so respected a member of the profession, soon collected around him some of the most eminent of his colleagues; and that their assiduity and anxious attention, on this melancholy occasion, could not be exceeded;—but it was in vain that they exerted their skill.

In the various relations of man, of husband, and of father, no one fulfilled his duties with more kindness. The expression of his feelings under a reserved, and at times a pensive deportment, conveyed to strangers perhaps an appearance of insensibility,—but those who knew him will bear testimony to the warmth as well as to the uniformity of his attachments. The mildness of his disposition, his forbearance, his constant serenity of mind, and his passibleness, were, perhaps, unexampled. As a physician, his humanity and disinterestedness knew no bounds. He has left, besides his respectable widow, an only son, who had just obtained the medical honours of the university of Pennsylvania; and who, in the remembrance of his fathers reputation and benevolence, will find ample encouragement to tread the arduous path of medical science.

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

METEOROLOGICAL OBSERVATIONS, &c.

STATE of the weather at Philadelphia during the first six months of 1820.

JANUARY.

Thermometer—Lowest at 8 A. M. 11. 1st day of the month.
Highest at 3 P. M. 40. 7th and 28 days.
Mean . . . . . 30.

Winds southerly—some heavy rains and several snows—measles continue—whooping-cough common.

FEBRUARY.

Thermometer—Lowest at 8 A. M. 14. 1st day of the month.
Highest at 3 P. M. 69. 24th day.
Mean . . . . . 35.

Winds, north-east and south-west—heavy rains—deep snow—thunder, lighting, and rain on the 16th—measles on the decline.

MARCH.

Thermometer—Lowest at 8 A. M. 30. 7th day of the month.
Highest at 3 P. M. 70. 25th day.
Mean . . . . . 50.

Westerly winds—clear weather—measles continue—ice islands met with in the Atlantic in this month.

APRIL.

Thermometer—Lowest at 8 A. M. 29. 3d day of the month.
Highest at 3 P. M. 87. 20th day.
Mean . . . . . 52.

Winds, north-east and south-west—snow on the 1st—clear, and some very warm weather—little rain—measles nearly extinct—rheumatism more common than usual—scarlatina frequent.

MAY.

Thermometer—Lowest at 8 A. M. 55. 16th, 17th, 18th days.
Highest at 3 P. M. 79. 1st and 23d days.
Mean . . . . . 60.

Westerly winds prevalent—clear until the end of the month, then heavy rain—scarlatina continues, and has proved mortal in several instances in this and last month.

JUNE.

Thermometer—Lowest at 8 A. M. 61. 12th day of month.
Highest at 3 P. M. 91. 30th day.
Mean 70. 70

Winds, north east and south west—generally clear weather—some heavy rains—warm season—several plentiful showers—though the drought was considerable—most abundant production of the fruits of the earth—a bilious remittent, often terminating in typhoid symptoms occurred in the last month and continued in this—not unusually mortal—chiefly confined to the black people—no small-pox.

The publication of our present number having been unavoidably delayed, we are enabled to insert the following interesting communication from Poulson's American Daily Advertiser.

A comparison of the mean heat of the two summers of 1819 and 1820, that is to say, from June 1st to August 15th in each year. The observations being made on the same thermometer, in the same situation and exposure, at a house in north Eighth street, in this city.—At sun-rise in the open air, and at 3 P.M. in a room on the second story of the house, with the window sash all fully raised, and the shutters half closed or bowed, as it is called.

	Sun-rise, out.	3 P. M. within.	Mean of both.
1819.			
June, - - - - - -	65.9	79.4	72.65
July, - - - - - -	68.4	80.8	74.6
Aug. 1 to 15, - - -	74	84.86	79.43
Mean of the 76 days,	68.5	81	74.75
1820.			
June, - - - - - -	63.36	78.1	70.06
July, - - - - - -	71.4	81.6	76.5
Aug. 1 to 15, - - -	70.7	81.3	76
Mean of the 76 days,	68	80.1	73 8

By the above statement it appears, that the mean heat of the 76 days, from June 1 to August 15, has been lower in

1820 than in 1819, nearly one degree, on an average, of sunrise and 3 P. M.; from which it might be inferred, so far as heat is an agent, that the health of our city should be better or more free from sudden mortality this season than the last.

Comparisons between one year and another ought always to be made on the same thermometer, in the same situations, otherwise the comparison will be of no use, as there is a difference of from one to five degrees at 3 P. M. in hot weather, between one situation and exposure and another, though both may be in the shade. C. E.

College of Physicians of Philadelphia, July 4, 1820.

The following officers of the College were duly elected.

President.—Doctor Thomas Parke.

Vice President.—Doctor Samuel P. Griffitts.

Censors.—Doctors William Currie; Henry Neill; Thomas T. Hewson; Edwin A. Atlee.

Treasurer.—Doctor Thomas C. James.

Secretary.—Doctor Samuel Emlen.

PRIZE MEDALS

Offered by the Humane Society of Philadelphia.

THE discoveries which have attended the recent labours of physiologists, have encouraged the Managers of the Humane Society to believe, that continued attention to the consideration of suspended animation, may be productive of benefit; they are therefore induced once more to offer,

For the best practical dissertation on the means of restoring persons apparently dead from drowning, *a gold medal of the value of three hundred dollars.*

For the second best dissertation, *a gold medal of the value of two hundred dollars.*

The dissertations may be written in the English, French, or Latin language, accompanied with a sealed paper, containing the author's name and place of residence.

They must be sent to the secretary of the Society, on or before the first day of the first month, January, 1822.

They shall be submitted to the judgment and decision of the College of Physicians of Philadelphia; and those which are not successful, shall be returned to their authors.

By order of the Managers.

J. WILSON, Sec'ry. JOSEPH CRUKSHANK, Pres.

Philadelphia, 6mo. (June) 14, 1820.

J. Wilson Moore Sec'ry

Obituary.

DIED, in Philadelphia, June 6, 1820, Doctor RENE LA ROCHE, aged sixty-four years.

DIED, at Montreal, on the 11th instant, after a lingering illness, which he bore with fortitude and christian resignation, Mr. FREDERICK PURSH, the celebrated botanist, aged forty-six years. He was a native of Russia. As a scientific and practical botanist his abilities were of the first order. Among his various publications, the American Flora, which he published in London in 1814, is esteemed by the learned both of Europe and America, as a standard work of the first authority.

He came to Canada for the purpose of exploring the country, and to collect materials for a Canadian Flora. This work, if his health had been spared, and from his known intention of exerting his utmost capacity and means to render it complete, useful, and splendid, would have been a great acquisition to those who feel interested in the productions of Canada. As a man, he was candid, liberal, and unassuming. His remains were followed to the grave by the members of the Horticultural Society, as a tribute of respect to his memory, as a distinguished member.

DIED, in England, on the 19th of June at his house, Spring-grove, near Hounslow, the venerable President of the Royal Society, the Right Hon. Sir JOSEPH BANKS, G. C. B., &c. &c. The loss to science by the demise of this excellent man and liberal patron will be long and severely felt. Sir Joseph had been for a long time labouring under a most distressing illness; for some years he had been deprived of the use of his lower extremities, and rendered so feeble as to be lifted from his room to his carriage. He possessed a princely fortune, of which he assigned a large portion to the encouragement of science, particularly natural history, private and public charities, and domestic hospitality.

London Medical Society, Bolt Court.

[From the London Medical Repository, for April, 1820.]

The following is the list of officers and council for the year 1820.

President.—Dr. Clutterbuck.

Vice Presidents.—Dr. Uwins; Dr. Blegborough; W. Norris, Esq.; John Abernethy, Esq.

Treasurer.—John Andree, Esq.

Librarian.—Dr. Hancock.

Secretaries.—Dr. Uwins; Thomas Calloway, Esq.

Secretary for Foreign Correspondence.—Henry Blegborough, Esq.

Council.—Dr. Babington; Dr. Walshman; Dr. Merriman; Dr. Pinckard; Dr. Copland; Dr. Blicke; Dr. Furnival; Dr. Ley; Mr. Johnson; Mr. Hooper; Mr. Lowdell; Mr. Bartlett; Mr. Leatham; Mr. Leese; Mr. Sutleffe; Mr. Drysdale; Mr. Pettigrew; T. Bryant; Mr. Dunlap; Mr. Hilliard; Mr. Harris; Mr. Shephard; Mr. Tarratt; Mr. Burrows; Mr. Ollier; Mr. Wigan; Mr. Morley; Mr. Good; Mr. Wray; Mr. Brown.

Chairmen of the Council.—Dr. Walshman; T. J. Pettigrew, Esq.

Library Committee.—Dr. Walshman; Dr. Copland; Dr. Furnival; E. Sutleffe, Esq.; T. J. Pettigrew, Esq.

To deliver the Anniversary Oration, 1821.—T. Calloway, Esq.

Registrar and Sub-Librarian.—James Field.

Hunterian Society.

The first annual meeting of this Society was held at the King's Head, Poultry, on the 10th of February; Sir William Blizard in the chair.

The following gentlemen are elected officers for the ensuing year:—

President.—Sir William Blizard, F. R. S.

Vice Presidents.—James Hamilton, M. D.; John Meyer, M. D.; Benjamin Robinson, M. D.; Lewis Leese, Esq.

Secretaries.—John T. Conquest, M. D. F. L. S.; William Cooke, Esq.

Council.—Thomas Bell, Esq., F.L.S.; Isaac Buxton, M. D.; Thomas Callaway, Esq.; W. D. Cordel, Esq.; R. Dunglison, Esq.; John Dunston, Esq., F. L. S., H. Greenwood, Esq.; William Kingdon, Esq.; Charles Meeres, Esq.; James Miles, Esq.; Benjamin Pierce, M. D.; Henry R. Salmon, Esq.

Communications on Medical and Physical topics, addressed to the Secretaries, No. 10, St Mary Axe, will be thankfully received.